THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. LII OCTOBER, 1939 No. 4

INTELLIGENCE LEVEL AND OCCIPITAL ALPHA RHYTHM IN THE MONGOLIAN TYPE OF MENTAL DEFICIENCY

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The study reported here is one of a series undertaken to determine the empirical relation between variations of intelligence and properties of the electro-encephalogram.[1] The term electro-encephalogram (E.E.G.) refers to the record that may be obtained of fluctuations of electric potential of cerebral origin by means of electrodes attached to the intact scalp. The report by Berger in 1929 that such electrical potentials may be detected without the need of operative procedures opened the way to a wide variety of studies upon the cerebral basis of psychological phenomena.[2] The present series of studies is concerned with the application of this technique to the investigation of cerebral factors on which intelligence may depend. The first step in determining the utility of the electro-encephalographic technique as an index of such cerebral factors would seem to be the determination of whether or not significant correlations may be found between intelligence and properties of the electro-encephalogram.

In the investigation of this question the feebleminded offer important methodological advantages: a wide range of variation in intelligence level, and the possibility of independently controlling the variables of mental age and chronological age. In the use of the mentally deficient in this

* Accepted for publication March 1, 1939.

[1] From the Research Department, Vineland Training School, and the Psychological Laboratory, Cornell University. This investigation was conducted during 1937 at the Vineland Training School. It was assisted by Grants in Aid from the Penrose Fund of the American Philosophical Society and from the National Research Council. Appreciation is expressed to Mr. Franklin W. Smith, laboratory technician, for his assistance in this study.

[2] For a discussion of the logical basis involved in the use of the E.E.G. as a psychophysiological indicator and for a survey of recent literature, see G. Kreezer, The electro-encephalogram and its use in psychology, this JOURNAL, 51, 1938, 737-759.

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series of studies, it has been considered advisable to deal with different clinical types in separate investigations. Such a procedure may be expected to decrease the danger of heterogeneous material masking and so preventing detection of any real relations that may obtain for any given type. The present study limits itself to the mongolian type of mental deficiency.[3]

One of the chief types of pattern exhibited in the human electroencephalogram is that which has been designated by Berger as the alpha rhythm.[4] It consists of rhythmical fluctuations of potential difference occurring at a frequency of about 10 waves a second, though with a certain range of variation in frequency for different individuals. This rhythm is generally most prominent at the occiput. In the present study, the specific problem undertaken was to determine what significant correlations obtained, if any, between intelligence level and certain properties of the occipital alpha rhythm in the mongolian type of mental deficiency. Reports of preliminary studies by the writer upon this question and incomplete reports upon the present investigation have been made in a number of earlier papers.[5] A detailed statement of experimental conditions, statistical treatment, and results has not, however, been made previously.

I. Experimental Conditions

The conditions involved in the present investigation may be conveniently summarized under the headings of subjects, apparatus, technique, analysis of records, and statistical procedures.

(A) Subjects. The *Ss* in the experimental group consisted of 50 individuals of the mongolian type of mental deficiency. These individuals were carefully selected

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[3] For a summary of data on this type of mental deficiency, see K. Brousseau, *Mongolism*, 1928, 1-210.

[4] H. Berger, Über das Elektrenkephalogram des Menschen, I, *Arch. f. Psychiat, u. Nervenkr.*, 87, 1929, 527-570.

[5] G. Kreezer, The electric potentials of the brain in the mongolian type of mental deficiency, *Psychol. Bull.*, 33, 1936, 607; Electric potentials of the brain in certain types of mental deficiency, *Arch. Neurol. u. Psychiat.*, 36, 1936, 1206-1213; Electrical phenomena of the brain among the feebleminded, *Proc. Amer. Ass. Mental Deficiency*, 42, 1937, 130-141. Other reports on the E.E.G. of the mentally deficient have been made by Berger (H. Berger, Über das Elektrenkephalogram des Menschen, *Arch. f. Psychiat. u. Nervenkr.*, 94, 1931, 49 f.; 98, 1932, 250-254) and, quite recently, by Rahm and Williams (W. E. Rahm and A. C. Williams, Aspects of the electroencephalogram in epilepsy and feeblemindedness, *Psychiat. Quart.*, 12, 1938, 230-235). Berger obtained exploratory records on a total of about 10 *Ss*. There is no indication that any of the *Ss* was of the mongolian type. In the interesting study of Rahm and Williams, a group of 35 unselected *Ss* were used, only two of whom were mongols. Since the bulk of the *Ss* were apparently of the undifferentiated type, the results of the study by Rahm and Williams may be more appropriately discussed in a forthcoming paper of the author's on the relation of intelligence level and the E.E.G. in the "non-differentiated familial type" of mental deficiency.

from the population of the Vineland Training School and the Vineland State School[6] to meet certain criteria with respect to evidence of mongolism, chronological age, and absence of certain pathological traits. Details concerning the criteria used and the composition of the group with respect to mental age are given below.

(1) Chronological age. *Ss* below 16 yr. of age or greater than 56 yr. of age were excluded from the experimental group. The upper age limit of 56 yr. was arbitrarily adopted as an aid in the exclusion of *Ss* who might have suffered pathological brain changes associated with senility. The lower limit of 16 yr. was adopted for two reasons: (1) to exclude possible effects of differences in chronological age on the alpha rhythm of the E.E.G.; and (2) to keep constant the factor of chronological age in intelligence level.

The only property of the alpha rhythm that has been reported as varying systematically with chronological age has been alpha wave frequency. This property has been found in normal children to increase as chronological age increases up to an age of between 8 and 11 yr., at which point it reaches the value characteristic of adults.[7] The adoption of a lower age-limit for our experimental group which is well above 11 yr. can be assumed to effectively exclude any influence of chronological age on alpha frequency.

Two indexes of intelligence level, derived from the Binet type of intelligence test, have conventionally been used by psychologists: the mental age—as an index of absolute level of achievement in the test, and the intelligence quotient (I.Q.) as an index of level of achievement relative to chronological age. In the computation of the I.Q. for adults, it is customary to take as the value for chronological age a standardized value not greater than 16 yr.[8] Consequently, the restriction of our experimental group to *Ss* older than 16 yr. means that the I.Q. of different *Ss* will be proportional to their mental ages, and independent of differences in chronological age. The classification of our *Ss* in terms of absolute mental age-level will, therefore, correctly represent their distribution in terms of intelligence quotient as well.

The larger proportion of the *Ss* in the experimental group fall between chronological ages 16 and 35 yr. The distribution in the total group was as follows: 16 to 25 yr., 27 *Ss*; 25 to 35 yr., 17 *Ss*; 35 to 45 yr., 4 *Ss*—one in each of the M.A.-groups indicated in Fig. 2, except the 5- to 6-yr. M.A.-group; and 45 to 56 yr., 2 *Ss*: one, 47.7-yr.-old, in the 1.5- to 3-yr. M.A.-group, and one, 56.0-yr.-old, in the 5- to 6-yr. M.A.-group.

(2) Criteria of mongolism. One of the principles employed in the current series of investigations of the relation of intelligence level to the E.E.G. has been that of dealing with different types of mental deficiency in separate groups. By this means we hoped to increase the chances of homogenity for any given experimental group,

[6] The Vineland Training School is a private institution for the care of mentally deficient individuals of both sexes. The Vineland State School is a New Jersey state institution for the care of mentally deficient girls. We are indebted to Mr. George W. Thorn, Superintendent of the Vineland State School, for his coöperation in this study.

[7] R. J. Smith, The electroencephalogram during infancy and childhood, *Proc. Soc. Exper. Biol. & Med.*, 36, 1937, 384-386; The electroencephalogram during normal infancy and childhood: II. The nature of the growth of the alpha waves, *J. Genet. Psychol.*, 53, 1938, 455-469; D. B. Lindsley, Brain potentials in children and adults, *Science*, 84, 1936, 34; Electric potentials of the brain in children and adults, *J. Gen. Psychol.*, 19, 1938, 288-291.

[8] R. Pintner, *Intelligence Testing*, 2nd ed., 1931, 84 f.

and to minimize the possibility of any real relations between the E.E.G. and intelligence level being obscured by the fact that different relations might conceivably obtain in different types of mental deficiency. As an extension of this principle, it was considered desirable to adopt some means of ensuring the selection of *Ss* all of whom could be regarded as good instances of mongolism. In most institutions, individuals may be found who might be regarded as representing intermediate or transitional forms of mongolism. Such individuals look like mongols in some respects, while in others they do not, and it becomes relatively difficult to state whether or not such individuals are to be classified as mongols.

To help exclude such 'intermediate' cases of mongolism and assure inclusion of only relatively marked cases of this clinical type, the following criteria were adopted: (1) the individual must have been designated as a mongol by the clinical staff of the institution of which he was a resident; and (2) he must show six or more of a set of 16 traits found in a study of Doll and Kuenzel to be most symptomatic of mongolism.[9] These 16 traits were: almond eyes, epicanthus, puffy eyelids, flat occiput, depressed nasal bridge, drooping mouth, thick lips, lips transversely fissured, dry facial skin, fissured tongue, thick tongue, pointed tongue, broad tongue, short fingers, dry nails and habitual use of "tailor posture" in sitting (hyperflexibility of joints). The examination of the various *Ss* with respect to these traits was made by the author of the present paper.

Since the report of the study by Doll and Kuenzel has not yet been published, it may be found helpful to state here those results of their study which suggested the use of the above criterion. In a survey of the literature of mongolism, these authors found a total of about 100 traits which have been reported as associated with mongolism by various writers, and a considerable lack of parallelism in the traits mentioned by different writers. They, therefore, set out to determine empirically which of this large set of traits could be considered most symptomatic of mongolism. Two groups of 30 individuals were selected, one group of mongols, and a control group of non-mongolian mental defectives, in which the individual *Ss* were equated for chronological age, life age, and sex with the mongolian group. These *Ss* were rated for presence or absence of the 100 odd traits by each of 4 judges familiar with the mentally deficient. The original list of traits was then reduced to 16 by the retention of only those traits which were found in 50% or more of the mongols and absent in 90% or more of the control *Ss*, by a total of any 3 out of the 4 judges. This group of 16 traits is listed in the preceding paragraph and was adopted in this study for rating of the mongols. Using this set of 16 traits, Doll and Kuenzel had the 4 judges rescore all the *Ss* in the mongolian and control groups in order to determine, among other things, the number of these 'mongolian-traits' which were present in all individuals of each of the two groups, as determined by agreement of 3 out of the 4 judges. They found, by this criterion, that every one of the mongols had 6 or more of the 16 traits, and that every one of the control-*Ss* had less than 2. On this basis, we have adopted the procedure of requiring all *Ss* in our experimental group to show six or more of the 16 'mongolian-traits' in order to be included in the group.

It is of interest that those *Ss* whom we found it necessary to drop from our experimental group (2 out of a total group of 52), on the basis of this criterion,

[9] Private communication. Appreciation is expressed to Dr. Edgar A. Doll for making available to us the results of his investigation.

were also those who seemed less distinctly of the mongolian type on the basis of total general impression. This selection of *Ss* for the experimental group was, of course, performed before an E.E.G. was taken for any given individual. The average number of mongolian-traits per individual for the total group was 8.9, with a range from 6 to 13.

(3) Pathological defects. All individuals who were blind, epileptic, or crippled in any way were also excluded from the experimental group.

(4) Sex. Ss of both sexes were included in the experimental group. The mongolian population of the institution was not such as to permit a constant proportion of each of the two sexes to be selected at each mental age-level. Statistical devices were therefore employed to adjust for any possible influence of sex upon the results. These are considered in the section on statistical techniques. The number of individuals of each sex at each of the mental age levels was as follows: M.A.-group 1.5 to 3 yr.: 2 males, 10 females; M.A.-group 3 to 4 yr.: 1 male, 10 females; M.A.-group 4 to 5 yr.: 4 males, 8 females; M.A.-group 5 to 6 yr.: 4 males, 6 females; M.A.-group 6 to 7.5 yr.: 3 males, 2 females; and total experimental group: 17 males, 33 females.

(5) Intelligence level. The intelligence level of all the *Ss* in the experimental group was measured by the 1915 form of the Stanford-Binet scale, supplemented by the Kuhlman-Binet for mental age test-levels under 3 yr. The mental age-scores for all individuals were on file as part of the regular clinical records of the two institutions. In cases in which a Binet mental age-score based on full-scale testing of the *S* had not been obtained within a period of 2 yr. prior to the taking of the electro-encephalogram, the *S* was retested by a member of the staff of the psychological clinic[10] within a period not greater than 0.4 yr. (5 mo.) after the taking of an electro-encephalogram.

In the case of all but 4 *Ss*, the chronological age of the *S* at the time of the intelligence test was greater than 16 years. In the case of these 4 *Ss*, the C.A. was between 14 and 16 yr., 2 of these being in the 3-4 yr. M.A.-group and 2 in the 5-6 yr. M.A.-group. The average interval between the Binet examination and the taking of the E.E.G. was 0.48 yr. for the group as a whole with a range between 0 and 2 yr. Since this interval was thus quite small, and in no case greater than 2 yr., and since available evidence indicates that only very small, if any, changes may occur in the M.A. of feebleminded *Ss* after a C.A. of about 14 yr.,[11] it was considered justifiable to use as the M.A. score for every *S* the score obtained at the time of the nearest Binet examination. These scores were thus regarded as correct at the time of the taking of the E.E.G., within the limits of errors of measurement of Binet mental age. The probable error of measurement of the M.A. scores obtained for the individuals in the experimental group may be assumed, on the basis of a recent study by Kreezer and Bradway,[12] to be of the order of 0.15 yr.

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[10] I should like to thank Dr. Anthony J. Mitrano and Miss Dorothy H. Basset, clinicians at the Vineland Training School and State School, respectively, for their coöperation in retesting subjects for whom retests were considered desirable.

[11] R. Pintner, *op. cit.*, 84 f.; E. A. Doll, *The Growth of Intelligence*, 1920, 1-130; F. Kuhlman, The results of repeated mental reëxaminations of 639 feebleminded over a period of ten years, *J. Appl. Psychol.*, 5, 1921, 195-224; L. Moore, Mental growth of low grade feebleminded, *Training School Bull.*, 26, 1929, 88-95.

[12] G. Kreezer and K. P. Bradway, The direct determination of the probable error of measurement of Binet mental age, *J. Educ. Research* (in press).

In estimating the extent to which changes in M.A. may occur after a chronological age of about 14 yr., 2 lines of evidence are relevant: (1) the studies indicating the most appropriate chronological age to be used in the calculation of I.Q. of adult *Ss*; and (2) the studies of changes in M.A. as a function of chronological age in feebleminded individuals. Pintner concludes, from a consideration of available evidence, that it is best to use 14 years as a basis for calculation of I.Q. for all individuals above this age.[33] The implication is thus that, on the average, growth in mental age stops at about 14 years of age. Special studies on the growth of intelligence in the feebleminded have been made by Doll, Kuhlman, and Moore.[34] Doll's study indicates that mentally deficient *Ss* whose final mental age does not exceed 8 yr. do not continue to show a significant growth in mental age beyond a chronological age of 12 yr., and that the age at which growth in mental age stops is still lower for the lower levels of the feebleminded. Moore's study of idiots confirms Doll's conclusion in indicating that the mental age of idiots ceases at a very early age, possibly under 10 yr. of age. Kuhlman concluded that the age of final arrest occurs somewhere between 15 and 18 yr., the lower grade groups tending to cease development earlier than the groups of higher intelligence level. The tables and graphs given by Kuhlman for the idiot and imbecile levels, which are the levels represented in the present study, indicate that the rate of increase beyond 14 yr. is extremely small, the maximum in any of the year-intervals represented being less than 0.16 yr. change in mental age in a single chronological year.

As a check on the correctness of our assumption that possible changes in M.A. occurring between the administration of the Binet scale and the taking of the E.E.G. record might be regarded as negligible and within the limits of errors of measurement, an effort was made to estimate the magnitude of the M.A. change that might have occurred in the interval. Kuhlman's data, as given in his Table I was used as a basis,[35] since these data suggest some possibility of significant M.A. changes at the chronological age levels represented by our *Ss*. In making these calculations, an effort was thus made to assume the worst possible situation with respect to the occurrence of possible errors. The procedure used in making these calculations consisted in first, determining from Kuhlman's Table I the increment (or loss) in average mental ages at successive chronological ages, for the idiot and for the imbecile groups. Kuhlman's data shows that for ages beyond that at which mental age-growth ceases, there may be slight losses in mental age-level. Ages greater than those given by Kuhlman were assumed to be associated with losses of the magnitude associated with the oldest age class given by him. On the basis of these data, the possible M.A. change that might have occurred for every subject in our experimental group, in the interval between M.A. examination and E.E.G. record, was calculated.

These changes we may refer to as "hypothetical interval errors." The average absolute magnitude of these "hypothetical interval errors" was 0.05 yr. with a range from 0 to 0.46 yr. Both plus and minus values were represented. In the case of only one *S* was the relation of the *S*'s M.A.-score and the hypothetical error such as would have led to the displacement of the *S* from one class interval to another in the scatter diagram used in calculating coefficients of correlation. In the

[33] Cf. footnote 11.
[34] Cf. footnote 11.
[35] Kuhlman, *op. cit.*, 199.

case of this *S*, the hypothetical error was —0.46 yr., the largest error found, and if assumed to be correct, it would lead to the displacement of the *S* from the 2.5 to 3.0-yr. class-interval to one class interval below. The effect of such a displacement on the coefficient of correlation of alpha index and M.A. would be to raise it very slightly, from a correlation of 0.31 to 0.32, a change that may be regarded as negligible. Equally negligible effects would be produced on other correlations. If we now consider the magnitude of the "hypothetical interval errors" indicated above in relation to the probable error of measurement of 0.15 yr. suggested earlier, it is apparent that they will fall within the limits of the errors classified as accidental errors of measurement. These considerations suggest, therefore, that the "hypothetical interval errors" involved in the measurements of M.A. fall within the limits of experimental error of measurement and that they do not lead to any appreciable effect on the results.

(B) Apparatus. The procedure involved in the recording of the electro-encephalogram is in principle simple. Extremely weak currents, generated presumably in the cerebral cortex of an individual are picked up by electrodes attached to the *S*'s head and transmitted to an amplifier to there produce fluctuating currents of like form but increased magniture. These magnified currents are conducted through an oscillograph and the form of the fluctuations recorded. The essential parts in the system are the electrodes with attached leads, the amplifier, and finally the oscillograph. Additional devices were used for purpose of calibration and exclusion of possible artifacts. This necessary equipment consisted of a photo-cell oscillator with attenuator for calibration of amplifier-oscillograph system, a filter for reducing the amplitude of muscle action potentials in certain subjects, and a 'Faraday cage' for shielding *S* from electrical currents of external origin. The details of the equipment used are given below.

(1) Electrodes. The electrodes used were of two kinds, the active or head-electrode, and the reference or ear-electrode. The head electrode consists of a flat coil of silver wire, 1 cm. in diam., lying on the under side of a small piece of sponge rubber which was held in place on the head by means of an elastic head band. Contact between the *S*'s skin and the silver coil was provided by means of saline electrode paste (Redux-Sanborn Electrocardiogram Co.). The rubber sponge over the electrode bulges down around the edges of the silver coil and thus provides a pocket within which is held the electrode and paste covering it. The ear-electrode consisted of a small rectangle of pure silver foil, bent over in a U-shape so as to make contact with both sides of the ear-lobe, and held in place by a spring ear-clip. Contact between the silver-surface and the skin was here too provided by means of the electrode paste. An electrode of this sort was attached to the lobes of both ears and connected together by wire leads to jointly provide the reference electrode.

The elastic head gear used for holding the active electrodes in place consisted of an elastic band attached around the circumference of the head (corresponding to a latitude-meridian of a globe) and two bands at right angles to the first (which corresponded to longitude-meridians). The length of all these bands may be adjusted to fit heads of different sizes, and the position of the 'longitude-bands' adjusted so an electrode may be located readily at any position on the head. The tension of the various bands was such as hold the electrodes stably in place without being great enough to be uncomfortable to the subject.

(2) Amplifier. The amplifier consisted of 5 stages, resistance-capacity coupled. A circuit diagram is shown in Fig. 1. The first stage is of the push-pull, balanced type, similar to that described by Matthews,[16] though with somewhat different circuit-constants. It was built as an independent unit, enclosed with its own batteries in a shielded metal case. It may be referred to as the pre-amplifier. The next three stages form a second unit, contained in a shielded metal case. This unit may be referred to as the voltage amplifier. The final, or power stage, forms a third physical unit, with separate batteries. Connections between the separate units are made by means of shielded microphone cable, and telephone plugs and jacks.

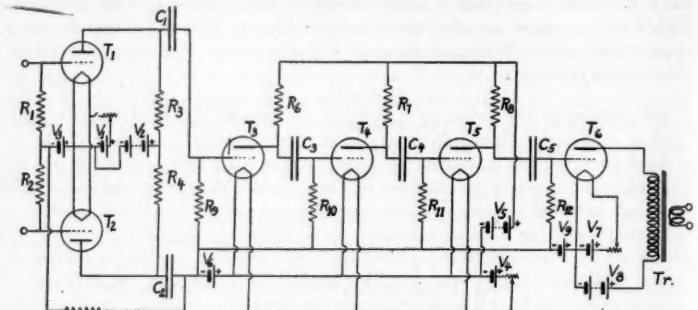

FIG. 1. CIRCUIT DIAGRAM OF AMPLIFIER

$R_1 = R_2 = 100{,}000$ ohms
$R_3 = R_4 = 10{,}000$ ohms
$R_6 = R_7 = R_8 = 150{,}000$ ohms
$R_9 = 250{,}000$ ohms
$R_5 = R_{10} = R_{11} = R_{12} = 1$ megohm
$C_1 = C_2 = 4$ μFd
$C_3 = C_4 = C_5 = 0.1$ μFd
$V_1 = 2$ volts (storage cell)
$V_2 = 67$ volts
$V_3 = V_6 = 1.5$ volts
$V_4 = V_7 = 6$ volts (storage battery)
$V_5 = 225$ volts
$V_8 = 135$ volts
$V_9 = 9$ volts
T_1, T_2 : W. E. 264 B
T_3, T_4, T_5 : W. E. 102 G
T_6 : 112 A
Tr.: Transformer G. R. Co. No. 541 C

An essential requirement of the amplifier apart from the provision of sufficient sensitivity and a satisfactory frequency-response curve was that it be free from disturbances which might introduce artifacts in the records. The disturbances to be excluded were of three kinds: (a) induction effects, or disturbances arising from electrical sources external to the amplifier; (b) microphonic effects, or disturbances produced by sound waves striking the amplifier tubes; and (c) internal noise-effects, or disturbances arising within the amplifier itself.

(a) Induction effects. Induction effects were eliminated through the use of adequate shielding for *S* and amplifier. The most serious source of disturbance in this category was the 60 ~ house current. A 'Faraday cage' provided shielding for *S*. It was constructed by lining all sides of *S*'s room with galvanized iron mesh. Since this amount of shielding was not found adequate to exclude all traces of the 60 ~ current from the records when *S* was connected to the amplifier (due to the relatively

[16] B. H. C. Matthews, A special purpose amplifier, *J. Physiol.*, 81, 1934, 28 P-29 P.

large capacity of *S* to ground), a second, smaller enclosure was built of fine copper mesh at one end of the room. This smaller enclosure was large enough to contain a cot for *S* and certain necessary apparatus. Additional shielding for the pre-amplifier and voltage amplifier units was provided by the metal containers in which they were built. These two units were located just outside of the small copper-lined enclosure containing *S* (in order that the leads from *S* might be kept short). All metal shields were connected together and grounded at the same point.

(b) Microphonic effects. The Western Electric 264B tubes of the pre-amplifier stage are relatively non-microphonic. As an added precaution, however, the bases on which these tubes were mounted were suspended from overhead brackets by means of springs. More complete measures were found necessary to shield the Western Electric 102G tubes used in the voltage amplifier. These tubes, though of the low noise type are moderately microphonic. The measures taken consisted in placing the voltage amplifier inside a galvanized iron box lined on the inside with a 1-in. thick layer of air-acoustic,[17] a sound absorbing material, and in suspending this box with the amplifier by means of springs from the brackets supporting it. The relatively large mass of the box gave it, in conjunction with the supporting springs, a low natural period of vibration (about one a sec.). Mechanical vibrations of higher frequency which were transmitted to it tended accordingly to be damped out. Tests showed that speech of moderate intensity in the immediate vicinity of the amplifier failed to produce microphonic effects.

(c) Internal noise. Various factors within the amplifier tend to produce small fluctuations in voltage which are referred to collectively as the noise-level of the amplifier. This noise-level sets the lower limit of useful amplification since signals which do not exceed it will be obscured by the fluctuations in voltage represented by the noise-level. The following measures were used to keep the noise-level low.[18] (1) Low-noise tubes were used in the pre-amplifier and voltage stages. These tubes are designed to reduce the factors in the tube (shot-effect, ionization, etc.) which may contribute to its noise-level. (2) The tubes in the first stage were operated at a relatively low plate-voltage. (3) A relatively low input resistance of 200,000 ohms was used to keep fluctuations due to the 'thermal effect' low, inasmuch as the magnitude of the 'thermal effect' is proportional to the input resistance.[19] (4) Coupling condensers and plate and grid leak resistors were selected so as to minimize noise-production that may arise in these components. The ordinary type of paper condenser was used only in the last stage, mica or oil-impregnated paper condensers being used in the earlier stages. Wire-wound resistors (non-inductive) were used in the pre-amplifier and voltage stages, since other types are likely to be noisy. The metallized type was found satisfactory for the last stage. (5) Care was taken to provide well-soldered joints for all connections within the amplifier. (6) The storage batteries which supplied the filament-current were kept well-charged, and the dry cells used

[17] This material was obtained from Johns-Manville Co. A layer 1-in. thick is reported as having a sound absorption coefficient ranging from one of 0.46 for a 128 cycle tone to a coefficient of 0.73 for a 2048 cycle tone (Bulletin No. VI of the Acoustical Materials Association).

[18] For a discussion of factors contributing to the noise-level of amplifiers, and procedures for reducing this level, see J. B. Johnson and F. B. Llewellyn, Limits to amplification, *Bell Syst. Tech. J.*, 14, 1935, 85-96.

[19] *Ibid.*, 85-88.

as B batteries were discarded when fall in output voltage indicated an increase in internal resistance. The best check on the dry batteries, was, however, a functional one. An increase in noise-level which was found not to be due to other factors could be eliminated by the substitution of fresh batteries. (7) In general, care with the early stages was most important inasmuch as disturbances arising at these stages are successively amplified by all later stages.

(3) Oscillograph. A Westinghouse PA bi-filar type oscillograph was used for recording. The unit used contains 3 galvanometer or 'vibrator' units, so that 3 records may be obtained simultaneously. One vibrator was used to provide a time-line, a second (standard type vibrator) was used to provide the records analyzed in this study, and the third vibrator (high sensitive vibrator) unit was used in conjunction with a second amplifier of modified design to record the very low frequency phenomena that may be present in the electro-encephalogram. In the records analyzed in this study, wave-frequencies below the alpha frequency range tended to be filtered out, as a consequence of the frequency-response curve of the amplifier, thus making it easier to analyze the records for different properties of the alpha rhythm. Records were made upon bromide paper (Eastman recording paper No. 697 or No. 797) moving at a rate of 22 mm. per sec.

The oscillograph is equipped with a polygon of mirrors and a viewing screen which permits the wave form of the phenomenon being studied to be observed visually without interfering with the taking of photographic records. This viewing attachment permits the nature of the potential fluctuations being picked up by the electrodes to be observed before a photographic record is taken. Artifacts such as an increased noise level, muscle potentials, or 60 ~ induction effects could, consequently, be detected, if present, and eliminated before a record was taken.

(4) Accessory devices: (a) Photocell oscillator.[20] A photo-cell oscillator was constructed for determining the frequency-response curve of the amplifier, that is, its relative sensitivity at different frequencies of input signal. In this type of oscillator, the form of the output current is regulated by the variations produced in the amount of light entering a slit in front of a photo-electric cell. A rotating disk placed between a light source and the slit can be cut in any desired shape and thus permit the form of the output current of the oscillator to be controlled. The frequency of the current fluctuations may be regulated by the frequency at which the disk is rotated. The use of a D.C. shunt-motor permitted this frequency to be readily adjusted. A gain control on the oscillator unit provided for adjustment of the output voltage.

The merits of this type of oscillator is that it makes available sinusoidal alternating currents or voltages in the low frequency range (for example, between 0 and 60 ~ a sec.) and that the amplitude of such alternating currents is independent of the frequency of the current fluctuations. It thus permits measurements of the relative sensitivity of the amplifier for the different frequencies in the low-frequency range (for example, between 5 ~ and 15 ~ a sec.) with which we are concerned in the present study.

(b) Attenuator. In order to reduce the output voltage of the oscillator to values of the range found in the electro-encephalogram, an attenuator was inserted be-

[20] We are greatly indebted to Mr. Albert A. Grass, resident engineer at the Harvard Physiological Laboratory, for the design of this oscillator.

tween the output of the oscillator and the input terminals of the amplifier. Inasmuch as the attenuator was to feed into a high resistance of 200,000 ohms, it could be constructed on the simple voltage divider principle. One of the attenuator units used was made with precision resistors with resistance ratings correct to within 1 per cent. With a 1.5-v. dry cell used as a source of voltage, this attenuator provided D.C. output voltages of 6, 30, 60, 300, and 600 microvolts (μv.). These values were determined by calculation. With the dry cell voltage assumed to be constant, errors in the calculated values of output voltage may be assumed to be no larger than that of the resistors. Another attenuator unit was used to provide a larger number of voltage steps in the low voltage range. Resistors of the inexpensive metallized type were used to provide the larger resistance values in this unit. The accuracy of the output voltages obtained was checked, through use of amplifier and oscillograph, against the outputs from the attenuator constructed with precision resistors. Any differences were within the limits of error with which deflections could be read on the oscillograph-record. Output voltages available with this unit, for 1.5 v. input, were 5, 10, 20, 50, 100, and 1000 μv. With each of the attenuator units, the different attenuation settings could be made readily by means of a selector switch.

The calibration of the output of the attenuator units in terms of the microvolt values specified above was based on the assumption of a 1.5-v. D.C. source. In using the *oscillator* with the attenuators to provide alternating voltages of equivalent magnitudes, it was necessary for its gain control to be adjusted so that the amplitude of the alternating voltage output of oscillator was 1.5 v. This adjustment was made by connecting the 1.5-v. battery to attenuator and amplifier-oscillograph system, and determining the deflection produced by D.C. pulses, for a given attenuator setting. The oscillator was now substituted for the 1.5-v. battery, and its gain control adjusted until the same deflection (from center line to peak) was produced on the oscillograph. This setting of the gain control could then be assumed to correspond to an output voltage of 1.5 v. The use of this method assumes that the frequency of the oscillator signal used lies in the region of maximum sensitivity on the frequency-response curve. That is to say, the amplifier must be equally sensitive to the D.C. pulses and the A.C. frequency provided by the oscillator. Otherwise equivalence of deflection would not imply equivalence of output voltage. The frequency used for the oscillator signal was 30 ~, a value lying well within the frequency region of maximum response of the amplifier, as shown by the frequency-response curve. As a further check on the equivalence of sensitivity of the amplifier to the 30-~ sinusoidal frequency and to rectangular pulses, a disk was made which permitted the oscillator to produce rectangular pulses instead of sinusoidal pulses. With all other conditions the same, the deflections produced on the oscillograph by these two types of oscillator-signal was found to be equal, within limits of errors of measurement. In using oscillator and attenuator to obtain a frequency-response curve for amplifier, it was, however, not necessary for the absolute value of voltage of alternating current signal to be known. All that was necessary was that the same voltage signal be used at all frequencies.

(c) Filter. A few *Ss*, although apparently lying quietly showed fairly persistent muscle potentials. These potentials were due presumably to a failure of *S* to relax sufficiently the muscles about the neck and jaws. Muscle potentials are of relatively high frequency, of the order of 40 or 50 waves a second. Though there is no

danger of their being confused with the alpha waves, which are much lower in frequency, they tend to obscure the alpha waves and render their analysis more difficult. A 'muscle-potential filter' was, therefore, used with some *Ss* to attenuate frequencies in the range between about 25 and 65 ~ a sec. The filter used consisted of a condenser and choke coil in series connecting the two leads just before the power stage. At a value of 42 H for the inductance and 0.13 μFd. for the capacitance, a frequency of 47 ~ was attenuated maximally, with frequencies on either side attenuated in lesser amounts corresponding to a slightly asymmetrical damped resonance curve. Thus, if, with filter inserted, transmission of the amplifier at 10 ~ is designated as 100%; transmission at 27 ~ is 68%; at 37~, 47%; at 47 ~, 37%; at 57 ~, 44%; and at 67 ~, 64%. In the alpha frequency range between 7 ~ and 14 ~, there was practically no change other than some decrease in absolute sensitivity, *e.g.*, at 10 ~ a change from 9.6 μv. per mm. to 12.7 μv. per mm.

(5) Performance of amplifier-oscillograph system. The performance of the amplifier-oscillograph system may be summarized in terms of the absolute level of sensitivity, the form of the frequency-response curve, time constant, linearity of response, and noise-level.

(a) Sensitivity. The standard sensitivity used corresponded to a 1-mm. deflection on oscillograph for 9.6 μv. input, at a sine-wave frequency of 10 ~ a sec. When the muscle-potential filter was used, the general level of sensitivity was somewhat lower, a 12.7 μv. input being required per mm. deflection at 10 ~.

(b) Frequency response. The frequency response-curve of the amplifier shows that as the frequency decreases from 100 ~ per sec., the curve is flat down to a frequency just below 20 ~, at which point the sensitivity begins to fall off slightly. The slope of the curve in the alpha frequency range with which we are concerned in the present study is indicated by the following figures. The sensitivities given are in terms of microvolts input required per mm. of deflection: at 20 ~ and above, 9 μv.; at 15 ~, 9.2 μv.; at 14 ~, 9.2 μv.; at 13 ~, 9.3 μv.; at 12 ~, 9.3 μv.; at 11 ~, 9.4 μv.; at 10~, 9.6 μv.; at 9 ~, 9.8 μv.; at 8~, 10.3 μv.; at 7 ~, 11.3 μv.; and at 6 ~, 12.7 μv.

(c) Time-constant. The time-constant of the amplifier (time required for deflection due to a direct current pulse to fall to 37% of its value) is 0.012 sec.

(d) Voltage response. The deflections of the amplifier-oscillograph system were linear with respect to input voltage up to the maximal input voltage used in the tests, above 300 μv. Tests were made both with D.C. pulses, and with A.C. sinusoidal signals and rectangular pulses produced by the oscillator. Deflections were measured from trough to crest. The amplifier was thus linear in response well beyond the range of input voltages indicated by any of the records, the maximum shown by any record being 108 μv.

(e) Noise-level. The noise-level of the amplifier was regularly determined with input terminals short circuited. At the standard sensitivity specified above, the base-line is practically a straight line, only a slight roughness in this line representing the noise-level. Records taken with a more sensitive oscillograph vibrator showed that the maximum amplitude of fluctuations present in the noise level was about 5 μv.

To determine the noise-level with an imput impedance equivalent to that present when an *S* was included in the circuit, a pair of the silver-coil electrodes were at-

tached to *S* on the skin over the tibia, at a separation of 5 cm. This region was selected as representing the region of the body surface most likely to be free of muscle potentials or other kinds of bio-electric potentials. The D.C. resistance between electrodes was 20,000 ohms, a value higher than the maximum shown by any of the *S*s during the experiment with electrodes connected to the head. The base-line obtained under these conditions was not distinguishable from that obtained with amplifier input short-circuited. The noise-level obtained with input short-circuited may therefore be regarded as representing the level of internal disturbance present in the amplifier when the *S* is included in the input circuit.

(C) Technique. The standard sequence of procedures carried out with each *S* was the following: (1) preliminary tests of amplifier performance; (2) attachment of electrodes to *S;* (3) measurement of inter-electrode resistance; (4) showing of preparatory motion pictures to facilitate adjustment of *S* to recording situation, and other procedures for preparation of *S;* (5) taking of records; (6) final showing of motion pictures; (7) measurement of inter-electrode resistance and electrode locations; and (8) final tests of amplifier performance. Details concerning these procedures are given below.

(1) Tests of amplifier-performance. To provide evidence of proper performance of the apparatus during the taking of all E.E.G. records, certain standard tests were made at the beginning and end of each recording period. These tests consisted in obtaining record of the noise level of the amplifier, and in testing amplifier for sensitivity with a 50 μv. signal. Two types of standard signal were used, a D.C. pulse and a sinusoidal 10-~ A.C.-voltage from oscillator.

(2) Location of electrodes. Active electrodes were applied at each of three regions of the head: over the occiput, the motor area, and the anterior frontal area, slightly to the right of the median plane. The results given in this paper are limited to the records obtained over the occiput. The standards adopted relative to the location of occipital electrode were based on two considerations: Adrian and Yamagiwa's report on the region of maximal alpha activity at the occiput,[21] and our expectation of a variation in size of the head of the subjects in the experimental group.

Adrian and Yamagiwa have reported that the locus of maximal alpha activity at the occiput extends over an area and is not limited to a specific point. This area is said to extend approximately 2 in. in a cephalic direction from the inion, and 2 to 2½ in. laterally in both directions from the medial plane. It may be inferred that an electrode located at any point within this area will be equally effective in the detection of the occipital alpha rhythm. It seemed likely, however, that the absolute limits of this area of greatest activity would vary somewhat with the size of the head. The occipital electrode was, accordingly, not located at a fixed distance from the inion and from the mid-line of all *S*s. An effort was made instead to adjust its location somewhat in accordance with apparent differences in the size of the head, without allowing it to fall outside the approximate limits given by Adrian as bounding the area of greatest alpha activity.

To permit subsequent statistical determination of the rôle of electrode position

[21] E. D. Adrian and K. Yamagiwa, The origin of the Berger rhythm, *Brain,* 58, 1935, 323-351.

on the results, measurements were made at the end of each recording period of head dimensions and electrode location. These locations could be determined from the white spots left on the scalp by the electrode paste, the center of the spots being taken to represent electrode-position. To facilitate measurements, two coördinate axes on the surface of the head were assumed: an x-coördinate axis represented by lines parallel to the median plane, and a y-coördinate axis, represented by lines perpendicular to the x-axis.

The following measurements of head-dimensions were taken (with tape measure): (1) distance from nasion to inion (nasion-inion distance); (2) the lateral distance across the vertex from left to right auditory meatus; and (3) the circumference of the head. Two measurements were sufficient to indicate position of any electrode: the distance from nasion to electrode along x-coördinate axis (nasion distance), and the distance from median plane to electrode along a y-coördinate axis (lateral distance).

On the basis of these measurements, each electrode-position was represented for purposes of comparison in terms of "absolute inion distance," "relative inion distance," "absolute lateral distance," and "relative lateral distance." These terms are defined as follows: (1) *absolute inion distance* equals the distance from inion to electrode—it was determined by subtracting nasion-distance of the electrode from the nasion-inion distance; (2) The *relative inion distance* equals ratio of inion distance to the nasion-inion distance; (3) the *absolute lateral distance* equals distance of electrode from medial plane; (4) the relative *lateral distance* equals the ratio of absolute lateral distance to one half the lateral distance between the two auditory meatuses. Statistical data on the relation of these aspects of electrode position to E.E.G. properties will be considered in the section on results.

(3) Inter-electrode resistance. Before attachment of electrodes, steps were taken to reduce the resistance of the skin at the proposed place of application. A little alcohol was used first to remove some of the skin-oils, and then a bit of electrode paste was rubbed into the skin with the tip of the finger. An extra dab of electrode paste was placed on the electrode before it was brought into contact with the skin.

The D.C. resistance between each of the active electrodes and the reference electrodes was measured by connecting in series with *S* a 1.5-v. dry cell and a milliammeter (1.5-ma. range), and sending a brief pulse of current through the circuit. Resistance could be taken as equal to voltage divided by current, on the basis of Ohm's law. The strength of current flowing was great enough to be easily read on the meter but usually insufficient to disturb *S* by stimulating cutaneous sense-organs.

The resistance varied from *S* to *S*, but was rarely greater than 10,000 ohms. In such cases in which the resistance was greater than that value, the electrodes were removed, and the place of application prepared by additional rubbing with electrode paste in an effort to reduce the resistance further. After the E.E.G. records had been taken, the inter-electrode resistance was again measured. The possible effects of differences in inter-electrode resistance on properties of the E.E.G. will be considered in the section on results.

(4) Subject-conditions during recording. In order to minimize special effects in the E.E.G. records which might be produced by varied physiological or psychological states in the subjects, an effort was made to establish standard conditions with respect

to muscular relaxation, elimination of visual stimulation, the removal of emotional excitement, and the avoidance of states of sleep or drowsiness.

(a) Muscle potentials. Muscular relaxation was considered desirable in order that the brain potential record might not be obscured by muscle potentials spreading from nearby muscle groups. Muscle potentials, exhibiting frequencies of the order of 50 to 60 fluctuations a sec., are in no danger of being mistaken for the much lower frequency alpha waves. They may, however, exceed the brain potentials in amplitude and thus interfere with their detection on the records. The only muscle groups found troublesome in this respect were those of the neck and the jaws. To facilitate relaxation of these muscles, the *S* was instructed to lie quietly on his back on a cot and to avoid mouth-movements during the recording period. If muscle potentials were still present, as observed in the viewing screen of the oscillograph, the *S* was observed for any apparent muscular tensions and instructed appropriately for the relaxation of particular muscles. In the case of a few *Ss*, for whom residual traces of muscle potentials were observed even after these measures were taken, records were taken with the 'muscle-potential filter' inserted in the amplifier before the power stage.

(b) Visual stimulation. Elimination of visual stimulation was effected by having the subject-room completely darkened during the taking of a record. Visual stimuli tend to bring about elimination or reduction in amplitude of the alpha rhythm, but the effect is not equally marked in all individuals. It was felt desirable, therefore, to obtain records of the various *Ss* when effects of visual stimuli were excluded. With individuals of a high enough mental age, visual stimuli can be most easily excluded by instructions to *S* to close his eyes. But among the lower grades of mentally deficient individuals used in this study, such instruction could not be relied on. A completely dark room was therefore used during the taking of records with all *Ss*.

(c) Emotional excitement. There has been some evidence that during states of emotional excitement, a given individual will not necessarily show the same type of brain-potential record that he will when such emotional states are absent.[22] It was therefore felt necessary in obtaining our basal records to take precautions to avoid states of emotional excitement on the part of the *Ss*, as represented, for example, by feelings of apprehension.

Factors which tended to prevent such feelings of apprehension arising in connection with the experimental situation were the fact that individuals in both institutions were generally well accustomed to going to the laboratory for tests of various kinds, and the fact that they had become acquainted with the *E* through visits of the latter to the various cottages. In preliminary studies, it was found, however, that some of the *Ss* became somewhat apprehensive when the room was darkened. To provide a situation which would make it seem natural to *S* to lie down in the dark, provision was made for projecting motion pictures on a screen placed above the lower end of the cot, at an angle such that the pictures could be seen most com-

[22] H. Berger, Über das Elektrenkephalogram des Menschen, VI, *Arch. f. Psychiat. u. Neurol.*, 99, 1933, 555-574; Recent studies of interest indicating an influence of emotional activity on the E.E.G. have been reported by Hoagland and co-workers (H. Hoagland D. E. Cameron, and M. A. Rubin, Emotion in man as tested by the delta index of the electroencephalogram, *J. Gen. Psychol.*, 19, 1938, 227-245, 247-261).

fortably when the *S* was lying down. When the *S* was brought from his cottage by *E*, he was told he was being taken to the laboratory to see moving-pictures. Consequently, when he was led into the experimental room, he would generally lie down on the cot without objection when told to do so in order to better see the motion pictures. After a 3-min. showing of a "Mickey Mouse" film, the projector was turned off and *S* told that if he would rest quietly for a short time, there would be more pictures later. If any body-parts seemed to be in uncomfortable positions or under excessive tension, a little adjustment by *E*, together with appropriate instructions, was usually effective in bringing about a more easy position. The E.E.G. records were then taken, and, at the end of the recording period, there would be another 3-min. showing of the film.

This procedure proved to be very effective in eliminating any externally observable signs of apprehension or unrest on the part of *S* in the experimental situation. It was a common occurrence for an individual, on being brought back to his cottage, to ask when he could come again to see the moving-pictures, or give other indications of having enjoyed himself. On subsequent visits of *E* to a given cottage, it frequently happened that individuals who had not yet served as *S* would ask to be taken to see the pictures. It seems quite safe to conclude that the records obtained were not distorted by effects produced by emotional unrest due to the experimental situation.

(d) Sleep. Investigations by a number of workers have demonstrated that as an *S* gradually falls asleep, distinct changes may appear in the electro-encephalogram. In the earliest stages of sleep, the change consists in a dropping out of the alpha waves, and there are signs that some diminution in amount of alpha activity occurs in even the preliminary drowsy stages of sleep.[28]

It would seem desirable, therefore, to have some assurance that the *S*s were not asleep during the taking of the records. We know of no way of obtaining an index of the degree of drowsiness of an individual, in the absence of an introspective report. The procedure used in obtaining the standard sequence of records for a given *S* was, however, of a sort to interfere with his falling asleep, and permitted the experimenter to observe whether there were any signs of *S* becoming sleepy. The various samples of electrical activity, obtained with *S*'s room dark, lasted each no more than 60 sec. After these intervals, a moderate light was turned on in *S*'s room, and *E* would enter for adjustment of the electrode switch which connected the electrodes attached to different regions of the head. During this switching period, *S* was talked to and observed, and signs of being awake elicited. The records obtained may therefore be assumed to be representative of the waking state of the various *S*s.

(5) Recording procedure. The Westinghouse oscillograph on which records were taken was located in a second dark room immediately adjoining *S*'s room. Through the use of the viewing screen, *E* was able to determine the nature of the electrical

[28] H. Berger, Über das Elektrenkephalogram des Menschen, III, *Arch. f. Psychiat. u. Nervenkr.*, 94, 1931, 16-61; A. L. Loomis, E. N. Harvey, and G. A. Hobart, Cerebral states during sleep as studied by human brain potentials, *J. Exper. Psychol.*, 21, 1937, 127-144; H. Blake and R. W. Gerard, Brain potentials during sleep, *Amer. J. Physiol.*, 119, 1937, 692-703.

activity being picked up by the electrodes. If muscle potentials were present, an effort was made to get *S* to relax more completely before a photographic record was taken. After each record, a light was turned on in *S*'s room, the electrode switch adjusted, and another record taken from an electrode located over another region of the cortex. The sequence of these records was: occiput, motor, frontal, and occiput. The first 3 records were taken with *S* in the dark during the entire period. In the course of the last record, after about 10 sec. had elapsed, a stimulus-light was turned on for 1 sec., and then after another 10 sec. interval, turned on for 1 sec. again. If a considerable amount of muscle potential was observed in the viewing screen during any record, it was repeated, after *S* had been observed and instructed to lie quietly. A careful protocol was kept of the sequence of records to eliminate all chances of error in their subsequent identification after development. The maximum duration of any record was 60 sec.

(D) Analysis of records. The E.E.G. records forming the basis for the present report were those obtained with active electrode over the right occiput. Details concerning location of electrode is given in the section on technique. The properties of the records which were measured were *alpha index, alpha wave frequency,* and *average alpha wave amplitude.*

For the purposes of this study, alpha waves were defined as those waves whose individual durations fell between limits of one-seventh and one-fourteenth of a second, the corresponding frequency range for such waves occurring in continuous sequences being 7 to 14 a sec. Waves falling within this duration or frequency-range were not, however, used for determining the alpha wave properties specified above unless they had an amplitude of 15 μv. or more, and occurred in sequences of 5 or more waves. Any such sequence was designated as a regular sequence.

The three properties of the alpha waves measured (alpha index, alpha wave frequency, and average alpha amplitude) may be defined as follows: Alpha index equals the percentage of a standard sample occupied by regular sequences of alpha waves; alpha wave frequency equals the number of alpha waves per second occurring within regular sequences; and average alpha wave amplitude is an estimate of the average amplitude of alpha waves occurring within regular sequences.

As a preliminary to the determination of these characteristics of the alpha waves, a standard 'readable' sample of 40 sec. duration (86 cm. with paper speed of 2.2 cm. per sec.) was selected. This selection was made after the record had first been surveyed and the limits of any sections containing artifacts marked on the record. Then the first 40 sec. of record exclusive of the intervals containing artifacts were regarded as a standard sample. The type of phenomenon which was rejected, as representing possible artifacts, were relatively high frequency waves occurring at frequencies of the order of 40 to 50 a sec., which were interpreted as muscle potentials, and single waves of long duration such as could be produced by movement of the head against the electrode. The standard sample was now examined and crayon or pencil marks made to indicate the limiting points of all regular sequences.

Alpha index could now be easily determined by measuring the total length of the standard record occupied by regular sequences, and dividing by the length of the

standard sample.[24] *Alpha frequency* was measured by counting the total number of waves occurring in regular sequences, and dividing this number by the total time occupied by all regular sequences. To determine *average alpha wave amplitude,* a sampling procedure was adopted, since it would have been excessively time-consuming to measure all alpha waves. This procedure was as follows: the 40 sec. of standard record was divided, by crayon marks, into 8 smaller units of 5 sec. each. As an index of the average amplitude of alpha waves, the plan envisaged was to measure in mm. the amplitude of the first 10 alpha waves in each of these eight 5 sec. sub-units, and to use the computed average amplitude of this sample of 80 waves as an index of the average amplitude of alpha waves throughout the entire standard record. This procedure was carried out in principle, but a number of supplementary rules were adopted for the following special situations. (1) If the beginning of a 5-sec. sub-unit fell in the middle of a 'regular sequence,' the waves used for measurement were those starting at that point, and not those at the beginning of the regular sequence. (2) If a total of 10 alpha waves (in a regular sequence) was not found in any 5-sec. sub-unit, then enough additional waves to make up the total of 10 were taken from the next following sub-unit containing alpha waves. The measurements for the next group of 10 waves representing this following sub-unit was then begun with the alpha wave following the last one measured, rather than with the first alpha waves in the sub-unit, since these would have already been measured. (3) In cases in which there was less than a total of 80 alpha waves in regular sequences throughout the standard record, the amplitude of all alpha waves was measured. In such cases, the average obtained was based on fewer than 80 waves. The average value obtained for the amplitude on the basis of this sampling procedure was converted from mm. to microvolts on the basis of the amplifier-calibration made at the time of a given *S*'s record. Although the sensitivity remained practically constant throughout the course of the study, very slight changes occurred, due probably to slight progressive changes in the condition of the batteries. A further correction was made in the recorded average amplitude, in accordance with the average frequency of the alpha waves found for a given *S*, adjustment being made to correspond to the slight differences in sensitivity of the amplifier at different frequencies specified in the section on the frequency-response curve.

(E) Statistical procedures: (1) Continuous variables. For determining the dependence of two variables on each other and for excluding the influence of a third variable, use was made, when the variables involved were continuous in character, of total and partial product-moment correlations. The statistical significance of

[24] In previous papers of the author, the term pattern was used to designate the percentage of time the alpha rhythm is present. In the present paper the term alpha index, proposed by Davis (H. Davis, The electroencephalogram, *Tabulae Biologicae,* 17, part 2, 1938, 126), is used instead. The criteria used by Davis in the computation of the alpha index differ somewhat from the criteria used in the present study. Davis' calculation of the "per cent time alpha" is based on sequences of alpha waves at least 7μv. in amplitude and occurring in sequences of 3 or more waves. The "per cent time alpha" reported in this and previous studies of the author is based on consideration of alpha waves 15μv. or more in amplitude and occurring in sequences of 5 or more regular waves.

these correlations was determined on the basis of the methods proposed by Fisher.[35] Fisher has pointed out that when the population for which a given correlation has been computed is relatively small, the determination of reliability on the basis of probable error is not valid, since the *r*'s will not be distributed normally. As a substitute, he has furnished tables which specify the size of *r*'s which might arise in uncorrelated populations, corresponding to different levels of probability and for different degrees of freedom of the population. He proposes as a criterion of significance the 0.05 level of probability. Thus, an obtained *r* is regarded as significant if it is greater than an *r* for which there is a probability of 0.05 of it arising in an uncorrelated population. In the case of obtained correlations which were found to meet this criterion of significance, the probability of the correlation arising in an uncorrelated population was computed more precisely on the basis of Fisher's procedures for transforming a given product-moment *r* into its corresponding *z* value.[36]

(2) Non-continuous variable. One of the variables considered in the present study, sex, is non-continuous in character. Determination of its relation to the alpha rhythm and correction for its effects, when found, could not therefore be made by way of product moment correlations. To determine whether sex showed any significant relation to properties of the alpha rhythm, two other methods, both described by Fisher, were used: (a) the *t*-method, for determining the significance of the difference between two means; and (b) the method for analysis of variance. Since certain differences from the procedures conventionally used were involved in the use of these methods in the present study, a brief description of the steps followed is necessary.

Obtaining the difference of the means for the two sexes, and testing the significance of this difference provides a direct method for testing the influence of sex on any given property of the alpha rhythm. It was not feasible, however, to carry out this procedure with the total experimental group as a single unit, in view of the need of excluding the possible effects of mental age. The female group in the present study contains a larger proportion of *Ss* of the lower mental age-levels, whereas the male group contains a relatively larger proportion of *Ss* of the higher mental ages; moreover, evidence to be presented later indicates a tendency for certain properties of the alpha rhythm to change with mental age. Consequently, in determining the influence of sex alone upon the alpha rhythm it was necessary to exclude possible effects of variations in M.A. To balance the effects of M.A.-level in the two sex groups, the practice was followed of comparing the means for the two sexes in each of the five M.A.-groups separately. The differences thus obtained for the various M.A.-groups were weighted in proportion to the number of *Ss* in the group, and combined to provide a final average difference to represent the total group of *Ss*. Differences due to M.A. were thus excluded from the final result. An estimate of the standard error of this difference was obtained by pooling the variances provided by the two sexes in the different M.A.-groups in the manner described by Fisher for the pooling of variances from different groups.[37] The ratio *t* of the average difference to the standard error of the difference could then be tested for significance by

[35] R. A. Fisher, *Statistical Methods for Research Workers*, 2nd ed., 1928. (The most recent edition is the 7th, which appeared in 1938.)

[36] Fisher, *op cit.*, 163-177.

[37] Fisher, *op. cit.*, 108.

means of Fisher's table of t, in the usual manner. For t to be regarded as significant it was considered necessary, in accordance with the practice proposed by Fisher, for the probability P corresponding to t to be less than 0.05.

A second method used was the method for "analysis of variance" described by Fisher. The term variance, it will be recalled, refers to the square of the standard deviation of a distribution of measures. The effect of a given factor, such as sex, on the dependent variable (the E.E.G. property under consideration) is determined by comparing the part of the total variance (of the E.E.G. measure) due to differences between the sexes with the part due to variations in the measurements within any sex group. A statistic z is computed which equals the $\log_e$ of the standard deviation due to sex variations minus the $\log_e$ of the standard deviation due to variations within each sex group. This statistic z can be tested for significance by means of tables of z given in Fisher's book.[28] The procedure used in the present study to exclude the influence of a third variable, such as M.A. consisted in dealing with each M.A.-group as a separate unit. The sums of the deviations squared obtained for the separate M.A.-groups are all pooled to provide the final statistics representing the total group, on the basis of which the variances due to sex and to errors of observation are computed. Since the variations due to M.A. do not enter into any of the statistics, the influence of M.A. may be regarded as excluded from the final result. Fisher points out that the "analysis of variance method" may be regarded as equivalent to the "t-method" when the independent variable has but two classes, as is the case for sex. Both methods have, nevertheless, been used in this study so that they may provide a check on each other. When the independent variable contains more than two classes, the method for comparison of means cannot be used. The method for analysis of variance can, on the contrary, be used with any number of classes.

The application of the methods described above to the mongolian group indicated significant effects of sex on alpha wave frequency and alpha wave amplitude. It therefore became necessary to exclude the possible influence of sex from the correlations obtained between alpha wave frequency and M.A., and between wave-amplitude and M.A. The familiar method of correcting such correlations by the obtaining of partial correlations, corrected for the third variable, could obviously not be used, since the influence of sex could not be represented by means of coefficients of correlation.

The following procedure was adopted for excluding the influence of sex from the correlation-coefficients in question. In connection with a given alpha wave property, for example, frequency, separate coefficients of correlation between the alpha-wave property and mental age were obtained for *each of the two sexes*. These r's were transformed into their corresponding z values on the basis of tables given in Fisher. These z values were then compared to determine whether they were significantly different. These tests indicated that the z values obtained could be regarded as representative of like-correlated populations. The z values corresponding to the two sex groups were then combined or averaged, and the resultant z-value for the composite group converted back into its equivalent r. The significance of the composite z-value, and consequently of the equivalent r was determined from tables.[29] The procedure just described thus provides a way for correcting for the effects of non-equal proportions of the two sexes in the different mental-age groups.

[28] Fisher, *op. cit.*, 177.

[29] Fisher, *op. cit.*, 163-171.

II. Results

(A) Dependence of properties of the alpha rhythm on Binet M.A.: raw correlations. The empirical relations obtaining between the properties of the alpha rhythm and Binet mental age were determined by means of product-moment coefficients of correlation. The coefficient of correlation obtained between alpha index and mental age was +0.316. The correlation obtained between alpha wave frequency and mental age was −0.291. The correlation obtained between alpha wave amplitude and mental age was +0.114. The size of the correlation required for statistical significance lies between 0.2875 and 0.2732, corresponding to 50 and 45 degrees of freedom respectively.[30] On the basis of this criterion, the correlations of M.A. with alpha index, and with frequency are significant whereas that between M.A. and amplitude is not significant. These estimates of significance are to be considered only provisional pending consideration of the possible rôle that variables other than M.A. may have played in these relationships. In the discussion that follows the correlations specified above between properties of the alpha rhythm and M.A. will be referred to as the raw correlations.

In Fig. 2, the dependence of the three properties of the alpha rhythm on mental age is represented graphically. Separate curves are given for the two sexes and, in addition, a third, a composite curve, which represents the total group of *Ss*, without consideration of sex. The extent to which sex should be taken into account in estimating the dependence of the alpha rhythm on mental age will be considered below. Points are also plotted in each graph to indicate the average values obtained for each property in a group of mentally normal adults made up of 15 males and 15 females.

For each of the properties of the alpha rhythm considered, the y-coördinates of the points on the curve are the mean values of the property in question for each of the mental age-groups. With respect to the x-coördinate axis, it should be noted that the M.A. class intervals represented are not the same as those used in computing correlations. In the computation of correlations, half-year M.A.-intervals were used. In the computation of the mean values represented in the curves in Fig. 2, a fewer number of mental age class-intervals was used in order to reduce the effect of sampling errors in any given group and thus permit the graphs to show more effectively any general trends in the data.

(B) The effect of other variables on the raw correlations. In Section I were specified the measures taken to control, by way of experimental conditions, or in the selection of *Ss*, certain factors which might have an effect on the alpha rhythm, independently of mental age and which might therefore have proven sources of error in the determination of the relation

[30] Fisher (Table V A), *op. cit.*, 176.

between the properties of the alpha rhythm and mental age. In the present section, the nature of some of these potential sources of error is examined, and an attempt made to determine whether or not they may be regarded as excluded from our results. The factors to be considered here are electrode resistance, electrode location, life age, and sex. These factors may be referred to, for the sake of brevity, as subsidiary variables. Our problem in

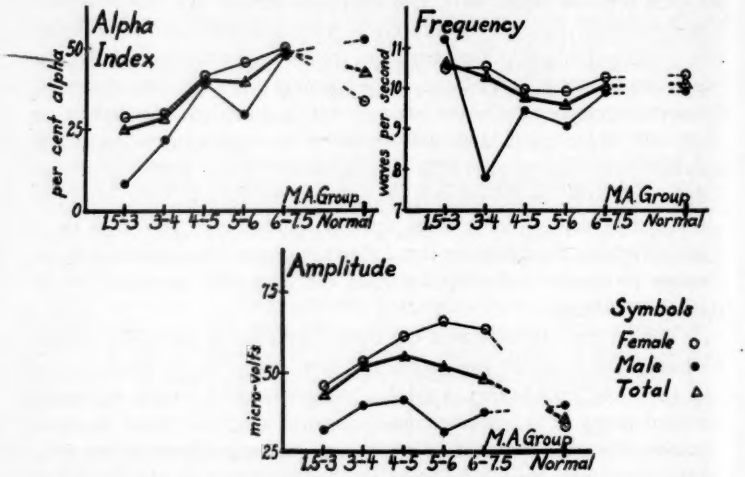

FIG. 2. PROPERTIES (MEAN VALUES) OF OCCIPITAL ALPHA RHYTHM AS A FUNCTION OF BINET MENTAL AGE-LEVEL

connection with each of these variables is the same. It is necessary (1) to determine whether any of them has a significant influence on the properties of the alpha rhythm, and (2) to correct for this influence when it is found to be present. Each of the properties of the alpha rhythm may be considered in turn.

(1) Alpha index and mental age. An examination of the relations between alpha index and each of the four subsidiary variables shows that none of them exercises a significant influence on alpha index. No adjustment is necessary, therefore, of the initial correlation of +0.316 obtained between alpha index and M.A. This correlation is greater than the value of 0.2875 required for significance. The probability of a correlation of this size arising by chance in an uncorrelated population is 0.03, or only 3 chances in 100. The correlation between alpha index and Binet M.A. may therefore be regarded as significant, and not dependent on any con-

comitant variations of electrode-resistance, electrode-location, life-age or sex. The detailed evidence is considered below.

(a) Electrode resistance. As indicated in the section on technique, a uniform procedure was followed in the application of electrodes, and measures were taken to assure good contacts of electrodes with the skin. Nevertheless, a certain range of variation in inter-electrode D.C. resistance was found among the various *Ss* of the group. It is necessary to inquire whether these variations could have influenced the correlation obtained between M.A. and alpha index, and to make corrections if they are found necessary.

Theoretically, an increase in inter-electrode resistance would have as a consequence a reduction in the voltage changes occurring at the input of the amplifier. The consequent reduction in the amplitude of the recorded waves might conceivably lead to a decrease in alpha index if waves of low amplitude were no longer counted on account of falling beneath the required minimum. The following calculations show, however, that even for the extreme values of resistance found among the *Ss*, the effect on the amplitude would be so small as to be negligible.

The average resistance found for all *Ss* in the group was 8480 ohms, with a range between 1170 ohms and 16,875 ohms. The maximum resistance, 16,875 ohms, was thus low relative to the input resistance of the amplifier, which was 200,000 ohms. An increase in inter-electrode resistance even as great as that between the two extremes of 1170 ohms and 16,875 ohms, could produce but a relatively small change in the amplitude of the recorded waves. Assuming a constant e.m.f. originating in the *Ss*, a change in resistance from 1170 ohms to 16,875 ohms could lead to a reduction in the amplitude of the recorded waves of but 7.3%. Accordingly, for alpha waves 1.5 mm. in amplitude from trough to peak, waves which just exceed the minimum required voltage of 15 μv., the reduction in amplitude would be less than 0.11 mm., a quite negligible difference. It seems improbable therefore that variations in electrode resistance could have had any appreciable effect on the alpha index.

These calculations are made on the assumption that the change in potential difference at the input of the amplifier will be proportional to the total resistance of the circuit consisting of the input resistance of the amplifier and the inter-electrode resistance in series. The calculations may be made on the basis of the D.C. measurements of resistance since the impedance of the input circuit to alternating current as low as 10 ~ will be but negligibly different from the D.C. resistance.

Inspection of the correlation coefficient actually obtained between alpha index and electrode resistance for the total group of *Ss* shows that electrode resistance had in fact no significant influence on the alpha index. The raw correlation between alpha index and electrode resistance was +0.008. The partial correlation between these 2 variables, corrected for variations in M.A. was —0.007. The correlations are thus extremely small and of no significance. (If, nevertheless, the correlation between M.A. and alpha index is corrected for electrode resistance, the change produced is from a correlation of 0.316 to a partial correlation of 0.324, an increase of 0.008.)

(b) Electrode position. As indicated in the section on technique, no effort was made to apply the active electrode at a constant distance from the inion and from the midline, but merely at the same approximate position. It is, therefore, necessary to inquire whether the slight variations which occurred in the position of the electrode exercised

any influences on the alpha indexes obtained. The position of the electrode may be characterized in terms of the absolute inion-distance, the relative inion-distance, absolute lateral distance, and relative lateral distance. The definitions of these terms have already been given on p. 516.

For the total group of *Ss*, the average absolute inion-distance was 1¼ in., with extreme values of 1 in. and 2 in. from the inion. The average absolute lateral distance was ⅞ in., with extreme values of ½ in. and 1⅜ in. from the median plane. As already noted, this electrode-region falls within the area specified by Adrian as representing the area of maximal alpha activity at the occiput. It is not to be expected, therefore, that the variations in electrode placement which occurred will show any significant relation to variations in the alpha index, in so far as alpha index may be dependent on the amplitude of the waves recorded.

An examination of the correlations obtained between alpha index and electrode position fails in fact to show any significant influence of electrode position within the limits involved. In the following list of correlations the first one given refers in each instance to the total correlation obtained; the second one to the partial correlation, corrected for M.A.

(1) Between alpha index and *absolute* inion-distance: —.13 and —.10;
(2) Between alpha index and *relative* inion-distance: —.13 and —.09;
(3) Between alpha index and *absolute* lateral distance: +.06 and +.16;
(4) Between alpha index and *relative* lateral distance: +.04 and +.18

The correlations are all well below the value of 0.287 required for significance. If, despite this evidence that variations in electrode position had no significant effect, the correlation of +0.316 obtained between *alpha index* and *M.A.* is corrected for electrode position, the following results are obtained. Corrected for *absolute* inion-distance, the partial correlation between alpha index and M.A. becomes +0.306; corrected for *relative* inion-distance, it becomes +0.303; corrected for *absolute* lateral distance, it becomes 0.347 and corrected for *relative* lateral distance, it becomes +0.328. The changes thus produced in *r* are relatively small, the partial *r*'s ranging between extremes of +0.303 and +0.347. All of these values are appreciably above the *r* of +0.287 required for significance. The presence of a significant correlation between alpha index and Binet mental age cannot, therefore, be attributed to the slight variations in electrode position. It seems appropriate, however, to regard the raw correlation coefficient of +0.316, in view of the evidence cited above that electrode position exercised no significant effect, as the most satisfactory value for representing the correlation between alpha index and M.A.

(c) Chronological age. As already noted, all *Ss* in the experimental group were older than 16 yr. of age chronologically, and thus well above the age at which any influence of life age on alpha rhythm has been reported. It is nevertheless of interest to determine whether variations in life age above the limit specified exercised any significant effect on the alpha index.

The product-moment correlation obtained between alpha index and chronological age was +0.008. The partial correlation obtained, when corrected for M.A., was —0.006. The correlations are thus extremely small, and of no significance. Variations in life age cannot, therefore, be considered a factor in the significant correlation obtained between alpha index and mental age.

(d) Sex. It was not found feasible, as pointed out on p. 507, to maintain the same proportion of the two sexes in the different M.A. groups. It is necessary, therefore,

to determine whether or not there are, for the present group of *Ss*, significant differences in the properties of the alpha rhythm of the two sexes and whether any statistical correction is required for differences in the proportions of the sexes represented at the different M.A.-levels.

As mentioned in the section on statistical method, two procedures were used for determining the influence of sex on the properties of the alpha rhythm, the method based on comparison of means, and the method based on analysis of variance. The following results were obtained from comparison of the mean alpha indexes of the male and female groups. The average difference in alpha index between males and females for the total experimental group is 11.01, the standard error of the difference is 8.24. The critical ratio t is 1.35. The minimum ratio required for significance even where the number of cases is large is 2. Since the obtained t was less than this, the obtained difference cannot be regarded as significant.

The application of the method for analysis of variance gave the following results. The parts of the variance compared was the part due to differences *between* the two sexes and the part due to variations *within* the two sex groups. The influence of M.A. variations was excluded by the procedure already described. The value for z obtained was 0.0899. The minimum z required for significance is 0.4311. The result thus agrees with that obtained above, in showing that sex exercises no significant influence on the alpha index, in the mongolian group. It is, therefore, not necessary for the correlation between alpha index and M.A. to be corrected for the effect of sex.

(2) Alpha wave frequency and mental age. When the relation between alpha wave frequency and the 4 subsidiary variables (*supra* 523) was examined, sex was found to be the only factor related significantly to alpha wave frequency. It is, therefore, necessary for the raw correlation of —0.291 obtained between alpha wave frequency and mental age to be corrected so as to exclude the possible influence of unequal proportions of the two sexes at different M.A. levels. The other subsidiary variables need not be taken into account further in view of the evidence just mentioned that they had no significant influence on alpha wave frequency. Possible effects due to sex may be excluded from the correlation between frequency and mental age by obtaining separate correlations for the two sexes, and averaging them by the method described by Fisher. The result of this procedure is to change the correlation coefficient between alpha frequency and mental age from one of —0.291 to one of —0.213. The latter coefficient may be regarded as more accurately representing the empirical dependence of alpha wave frequency on mental age. This coefficient is *not,* however, statistically significant since it is smaller than the minimum coefficient of 0.2875 required for significance. The chances of a correlation as large as 0.213 arising by chance in an uncorrelated population are 15 in 100 (as determined through the use of Fischer's z coefficients); whereas for r to be significant, the chances should not be greater than 5 in 100. It may be concluded, therefore, that there is no adequate evidence of a real association between alpha wave frequency and Binet

mental age. The detailed results supporting the above summary follow.

(a) Electrode resistance. The correlation obtained between alpha wave frequency and electrode resistance was —0.263; the partial correlation corrected for variations in M.A. was —0.205. Neither of these correlations are significant inasmuch as the minimum correlation required for significance lies between 0.2875 and 0.2732, corresponding to 45 and 50 degrees of freedom, respectively. The number of degrees of freedom in our sample is 48 in the case of the total correlation, and 47 in the case of the partial correlation. On the basis of the criterion of significance adopted, it may be concluded that variations in electrode resistance had no significant influence on alpha wave frequency.

(b) Electrode position. The correlations obtained between the various components of electrode position and alpha wave frequency were as follows:

Between frequency and absolute inion distance	—.097	and —.142
Between frequency and relative inion distance	—.045	and —.090
Between frequency and absolute lateral distance	—.074	and —.170
Between frequency and relative lateral distance	+.007	and —.100

In each case, the correlation first given is the total correlation; the second is the partial correlation, corrected for variations in M.A. None of the correlations given is significant since they all fall well below the criterion of significance specified above. It may be concluded that variations in alpha wave frequency were independent of any variations which were present in absolute or relative electrode position.

(c) Chronological age. The correlation obtained between alpha wave frequency and chronological age was +0.153; the partial correlation between these two variables was +0.174, corrected for variations in M.A. Neither of these correlations are significant. It may therefore be concluded that there is no significant association between variations in alpha wave frequency and chronological age, for a range between 16 and 56 yr.

(d) Sex. The data resulting from the comparison of means for the two sexes (with the influence of M.A. excluded) were as follows. The average difference in alpha wave frequency for the two sexes was 0.764 waves per sec. with a standard error of 0.34. The sign of the difference corresponds to a higher frequency for the female group. In the determination of this difference it was found that the female group showed the higher mean frequency for every one of the five M.A.-groups considered except the lowest. The ratio t of the average difference to its standard error is 2.24. The number of degrees of freedom represented is 40. Fisher's t-table indicates that for 30 degrees of freedom, the t required for significance is 2.042. On this basis the average difference found of 0.764 may be regarded as significant.

The application of the 'analysis of variance' method, with the influence of M.A. excluded, gives the following data. The value of z obtained was +0.9101. The value of z required for significance is 0.4648 for the 0.05 level of significance, and 0.654 for the 0.01 level of significance. The z obtained must therefore be regarded as significant and indicates a real effect of sex upon the frequency of the alpha waves in the mongolian group.

The two statistical tests applied thus agree in indicating a significant influence of sex on alpha-frequency in the mongolian group, and points to the need of

excluding the possible effects of sex in the determination of the relations between frequency and mental age. To exclude the influence of sex from the correlation between alpha wave frequency and mental age, we may obtain separate correlations between these latter two variables for each of the two sexes, and average them in the manner described by Fisher.

The steps in this procedure are as follows: The correlations obtained between wave frequency and M.A. are —0.198 for the female group, and —0.261 for the male group. These values are converted into their corresponding z values by means of Fisher's Table V B, and the difference between the z's tested for significance. The difference between the z values was found to be 0.053 with a standard error of 0.348. The difference is not significant and the two r's may be regarded as derived from equally correlated populations. It is, therefore, justifiable to average them.

The (weighted) average value of z was found to be 0.2168 with a standard error of 0.151. The value of r corresponding to this average z is 0.213. The ratio of z to its standard error equals 1.44. Since this ratio is less than two, z cannot be regarded as significant. We may obtain a more precise estimate of the probability of the z obtained arising in an uncorrelated population by the use of a normal probability table. Corresponding to the ratio 1.44 just given, it is found that the chances of a z as large as 0.2168 arising in an uncorrelated population are 15 in 100. The chances are, therefore, 15 in 100 that an r as large in absolute magnitude as 0.213 could arise by random sampling in an uncorrelated population. This value is appreciably greater than the value of 0.05 allowed by our criterion of significance. We are not justified, then, in inferring a significant relation between alpha wave frequency and mental age.

(3) Alpha wave amplitude and mental age. An examination of the relation of the four 'subsidiary variables' and alpha wave amplitude reveals the fact that significant differences in wave-amplitude are associated with differences in sex in the mongolian group. It is therefore necessary in this instance, too, to correct the correlation of alpha wave-property and mental age to exclude possible errors due to the sex variable. The other three variables considered, electrode resistance, electrode position, and life age, show no significant correlation with alpha wave amplitude and therefore make no corrections necessary. When potential errors due to sex are excluded, by obtaining correlations for the two sexes separately and combining them, the correlation between wave amplitude and mental age is changed from a value of $+0.114$ to one of $+0.313$. By the use of Fisher's z-coefficient it is found that the probability of an r of this size arising by chance in an uncorrelated population is 0.031. The probabilities of an r as large as 0.313 occurring by chance are thus less than the 0.05 value adopted as a criterion of significance. It may be concluded, therefore, that alpha wave amplitude shows a significant positive correlation with mental age; that is to say, wave amplitude tends to increase with mental age level. The detailed results follow.

(a) Electrode resistance and wave-amplitude. The correlation obtained between electrode-resistance and wave-amplitude was —0.183; the partial correlation corrected for variations in M.A. was —0.22. These values are both below the minimum value of 0.287 required for significance. It may be concluded that electrode resistance has no significant influence on alpha wave-amplitude.

(b) Electrode position and wave-amplitude. The correlations obtained between the various components of electrode position and alpha wave-amplitude were as follows:

Between amplitude and absolute inion distance	—.115	and —.105
Between amplitude and relative inion distance	—.064	and —.049
Between amplitude and absolute lateral distance	+.005	and +.040
Between amplitude and relative lateral distance	+.095	and +.142

The correlation given first in each line is the total correlation, that given second is the partial correlation, corrected for M.A. All of these correlation coefficients are well below the minimum value of 0.287 required for significance. It may be concluded that variations in electrode-position exercised no significant effect on alpha wave-amplitude.

(c) Chronological age. The correlation obtained between wave amplitude and chronological age was —0.044; the partial correlation, corrected for M.A., was —0.049. Both of these correlations are extremely small, and obviously not significant. It may be concluded that variations in chronological age within the limits involved in this study were not associated with variations in alpha wave amplitude.

(d) Sex and alpha wave-amplitude. The investigation of the influence of sex on wave amplitude carried out by way of the comparison of mean values in the two sexes gave the following results: average difference between the two sexes (female minus male), with influence of mental age eliminated, is +20.20; the standard error of this difference is 7.69; the ratio t of the difference to its standard error is 2.6; the number of degrees of freedom involved is 40. The value of t required for the 0.05 level of significance for 30 degrees of freedom is 2.042; t is thus well above this value and must regarded as significant. These results justify the conclusion that there is a significant tendency in the mongolian group for the females to show alpha waves of greater amplitude than the males. It is of interest to note that the mean value of the amplitude was greater for the females than the males in each of the five M.A.-groups.

The application of the 'analysis of variance' method in order to determine the influence of sex on wave-amplitude gave the following results. The value of z obtained was +0.5099; the value of z required for significance for the 0.05 level of significance was 0.4648. The value of z obtained is higher than this value and consequently must be regarded as significant. The results of this test thus agree with the other in indicating that sex exercises a significant influence on alpha wave amplitude.

It accordingly becomes necessary to correct the value of the correlation coefficient obtained between wave-amplitude and mental age, in order to eliminate effects due to unbalanced proportions of the sexes at different M.A.-levels. The procedure used previously of obtaining separate coefficients of correlation for each of the sexes and averaging them may be used. The data obtained were as follows. The first step is to obtain coefficients of correlations for the two sexes separately and to deter-

mine whether they may be regarded as arising from equally correlated populations. The coefficients of correlation obtained between alpha wave amplitude and M.A. was +0.400 for the females, and +0.044 for the males. The corresponding z values were 0.4183 and 0.0433 respectively. The difference 0.375 was found to have a standard error equal to 0.348; the ratio is accordingly 1.07. The value is below the minimum critical ratio of 2 required for significance. The result is thus compatible with the hypothesis that the r's for the two sexes were derived from equally correlated populations.

The next step is to combine the z values corresponding to the two sexes. The two z-values are weighted in proportion to their variances and averaged. The result is an average z for the total group equal to 0.324 with a standard error of 0.150. The ratio of z to its standard error is 2.15. Since this ratio is greater than 2, z and its corresponding r (which equals 0.313) may be regarded as significant. Or we may determine more precisely the level of significance of z through the use of a normal probability table. The table shows that a deviate which is 2.15 times its standard error has a probability of being exceeded of but 0.0316. This value is smaller than our 0.05 criterion of significance. It may be concluded, therefore, that the average value of r, equal to 0.313, which we have obtained to represent the total group, is significantly greater than zero, since the chances of its arising by random sampling in an uncorrelated population are but slightly more than 3 in 100. There is thus evidence of a real association between alpha wave amplitude and mental age, when the effect of an uneven proportion of the two sexes at different mental age levels is eliminated.

It is of interest to consider in more general terms why the elimination of possible errors due to unequal proportions of the sexes has *increased* the size of the correlation between amplitude and M.A. The initial correlation of 0.114 obtained for the total experimental group represents a population in which there are relatively more females at the lower M.A.-levels, and relatively fewer at the upper M.A.-levels. Since the females tend to show greater alpha wave amplitudes than the males, the effect of such disproportions will be to raise the level of regression line (of amplitude on M.A.), at the lower mental age-levels and lower it at the higher levels. The slope of the line is thus less than if there were the same proportions of females at all M.A.-levels. Consequently when we eliminate the influence of such disproportions by obtaining correlations for each sex group separately, the result will be an increase in the positive slope of the regression line and in the corresponding coefficient of correlation.

A similar type of reasoning applies in accounting for the *reduction* which takes place in the correlation coefficient between alpha wave frequency and M.A. when the sex factor is controlled. In this case we are dealing with a negative correlation and a regression line of negative slope. Results cited earlier showed that the female group tended to show higher alpha frequencies than the male groups. The excessive proportion of females at the lower M.A.-levels should, therefore, have the effect of raising the level of the lower end of the regression line, that is to say, of increasing the negative slope of the line. Consequently when the effect of a disproportion in the sexes at different M.A.-levels is eliminated by dealing with each sex-group separately, the regression line will acquire a flatter slope and the coefficient of correlation will become smaller in absolute magnitude.

III. Summary and Conclusions

The present study represents one of a series undertaken to determine whether there are significant correlations between the electro-encephalogram and intelligence level. The feebleminded offer important methodological advantages in the investigation of this question, since they provide the possibility of a wide range of variation in intelligence level, and permit independent control of mental age and chronological variables. Properties of the electro-encephalogram examined were the alpha index, alpha wave frequency, and alpha wave amplitude of the occipital electro-encephalogram. The experimental group was limited to *Ss* of the mongolian type of mental deficiency. Intelligence level was measured by means of the 1916 form of the Stanford-Binet Scale, supplemented by the Kuhlman-Binet Scale for test levels under 3 yr.

Considerable attention was given to subsidiary conditions which might have led to changes in the electro-encephalogram independently of mental age effects, or which might have obscured the characteristics of any record. The conditions of this sort taken into account were instrumental artifacts, muscle potentials, conditions in *S* during recording, such as states of sleep, emotional excitement, and visual stimulation, variations in electrode position, inter-electrode resistance, chronological age-variations, and the proportions of the two sexes at different mental age-levels. Procedures for control of such factors occurred by way of selection of *Ss*, control of experimental conditions, techniques of record-analysis, and statistical corrections in the treatment of data.

The final results obtained after steps were taken to exclude possible errors due to such factors are represented by product-moment correlations between mental age-level and alpha index, alpha wave frequency, and alpha wave amplitude. Statistically significant correlations were obtained of mental age-level with alpha index and alpha wave amplitude, but not with alpha wave frequency. The criteria of statistical significance used were those of Fisher.

The finding of the significant correlations just mentioned points to the existence of common physiological factors underlying certain properties of the electro-encephalogram and mental age-level in the mongolian type of mental deficiency, and opens the way to the use of the electro-encephalogram in the discovery of the nature of these factors. Before these results can be regarded as definitely established, however, they must be confirmed by further, more extensive investigations, particularly in order to determine the extent to which errors of sampling may have been involved in the selection of the experimental group. More extensive investigations to this end are in progress.

AN EXPERIMENTAL STUDY OF REVERSALS IN READING

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During the past three decades considerable clinical and experimental attention has been devoted to children with average or superior intelligence who have had difficulty in learning to read under ordinary school instruction.[1] A characteristic noted in such children has been the frequent reading of words, letters, or sentences in a reversed or right-to-left direction. Implicit in many of these studies is the assumption that children making reversals actually observe the reading material in a right-left direction. Theories have been advanced to account for the presumed backward visual observation pattern.[2] Experimental evidence to show that the visual impressions leading to the reversal-responses were formed in the right-left direction, however, has been lacking.

The aim of the study reported here was to investigate reading behavior

* Accepted for publication September 15, 1938. This paper is the first of three covering a dissertation presented in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Yale University in 1935. The study was done under the supervision of Professor Walter R. Miles and Dr. Catherine C. Miles to whom the writer is greatly indebted.

[1] Extensive bibliographies and summaries of the literature on reading disability have been compiled by A. I. Gates (*The Improvement of Reading*, 1927, 1-440), W. S. Gray (*Summary of Investigations Relating to Reading*, 1925, 1-275; Summary of reading investigations, *J. Educ. Res.*, 26, 1933, 401-424), and Joseph Jastak (Interferences in reading, *Psychol. Bull.*, 31, 1934, 244-272). Experimental studies on reversals have been reported by W. F. Dearborn (The nature of special abilities and disabilities, *School and Soc.*, 31, 1930, 632-636), L. G. Fildes (Psychological inquiry into the nature of the condition known as congenital word blindness, *Brain*, 44, 1921, 286-307), A. I. Gates and C. C. Bennett (Reversal tendencies in reading, *Teach. Coll. Bur. Publ., Col. Univ.*, 1926, 1-23), Gertrude Hildreth (Success of young children in number and letter construction, *Child Develop.*, 3, 1932, 1-14; Reversals in reading and writing, *J. Educ. Psychol.*, 25, 1934, 1-20), Alexander Mintz (A study of indications of unstable unilateral cerebral dominance, reading disability and mental deficiency, *Psychol. Bull.*, 30, 1933, 565-566), Marion Monroe (Methods for diagnosis and treatment of cases of reading disability, *Genet. Psychol. Monog.*, 4, 1928, 335-456; *Children Who Cannot Read*, 1932, 1-205), Lorene Teagarden (Tendency to reversal in reading, *J. Educ. Res.*, 27, 1933, 81-97), Clifford Woody and A. J. Phillips (The effects of handedness on reversals in reading, *J. Educ. Res.*, 27, 1934, 651-662), and others. Many clinical observations of the phenomenon have also been reported.

[2] W. F. Dearborn, Teaching reading to non-readers, *Elem. School J.*, 30, 1929, 266-269; Structural factors which condition special disabilities in reading, *Proc. Amer. Assn., Ment. Def.*, 57, 1933, 268-283; The causes of "word-blindness," *Science*, 80, (no. 2084), 1934, 12-14. S. T. Orton, Specific reading disability—strephosymbolia, *J. Amer. Med. Assoc.*, 90, 1928, 1095-1099; The three levels of cortical elaboration in relation to certain psychiatric symptoms, *Amer. J. Psychiat.*, 8, 1929, 647-659.

involved in making reversals. The reading responses of children inferior in reading ability were observed under carefully arranged experimental conditions and then compared with those of average readers obtained under the same conditions.[3] Six experiments were conducted. Four were concerned with the effect on word-reversals (I) of controlling direction of word-exposure, (II) of eliminating reactive eye movements, (III) of accentuating word-form, and (IV) of mirroring the material. The remaining experiments were concerned with (V) the direction of reading single letters and rows of letters, and (VI) the direction of reproducing geometric designs.

Method and Procedure

Subjects. Two groups of *Ss* were selected for the experiments: a control group, composed of *Ss* showing average achievement in reading and arithmetic computation (reading-age and arithmetic-age approximating chronological age closely enough to yield quotients between 90 and 110), and an experimental group, composed of *Ss* possessing average achievement in arithmetic computation but a retardation in reading-age of at least two years below chronological age. The groups were further limited by selecting *Ss* who were within arbitrary ranges of certain variables with which reading achievement has been demonstrated to accord fairly closely: (1) average intelligence; (2) chronological age between 8 yr. 11 mo. and 9 yr. 11 mo.; (3) attendance in school for three complete years beyond kindergarten; (4) absence of speech disturbances; and (5) membership in unilingual, American-born families of middle social and economic status. Finally, the *Ss* of the experimental group were paired with those of the control group on mental and chronological ages.

The study was carried out in five grade schools in Weathersfield, a commuters' suburb of Hartford, Connecticut, inhabited chiefly by families of middle social class, and in Ivy school of New Haven, Connecticut, which is attended chiefly by children from middle social class homes. All children who were in the fourth grade or below and who complied with the chronological age requirement and the arbitrary limits listed above were given one form of the New Stanford Achievement Paragraph Meaning Test and Arithmetic Computation Test. The children who approximately fulfilled the achievement requirements were then given the Haggerty Sigma I Reading Test and a second form of the New Stanford Paragraph Meaning Test and Arithmetic Computation Test.

The children who were indicated by the above mentioned tests as having a special reading difficulty of the degree defined for this study were given the Stanford Binet Intelligence Test. Those individuals who now met the requirement of average intelligence and who confirmed the manifestation of reading difficulty during an individual examination on the Gates Pronunciation Test and a third form of the Stanford Achievement Test were enlisted in the experimental group.

[3] In subsequent articles the two groups of *Ss* used in the present study of reversals will be compared with respect to certain other factors which have been considered by some investigators as typical of, or causal in, reading disability and reversal-errors; hand and eye dominance, audition, vision, verbal associations, and emotional adjustment.

The control group was selected in an equally rigorous manner. Stanford Binet tests were administered to children from the total group surveyed whose achievement test scores met the criterion of average adequacy and whose chronological ages could be paired with those of the experimental *Ss*. Individuals were selected whose mental ages matched those of the experimental *Ss*.

Of the 119 boys and 99 girls tested in the original survey, 21 of the former and 4 of the latter proved to have a reading difficulty of the degree defined for this study. Eighteen of the boys and 2 of the girls conformed with the other criteria also. In the interests of homogeneity the present report has been confined to the results obtained from studying the boys.

The grade placements and number of grade repetitions for the 18 experimental and 18 control boys are given in Table I. All *Ss* had attended school 3 years beyond kindergarten; all, consequently, had been exposed to reading instruction for an equal number of years. The majority of the control *Ss* were in the fourth grade; the majority of the experimental ones, having repeated a grade, were in the third grade.

TABLE I

CLASSIFICATION OF THE *Ss* ACCORDING TO GRADE-PLACEMENT AND NUMBER OF GRADES REPEATED

Ss	Grade placement			Number grades repeated		
	2	3	4	0	1	2
Experimental	1	15	2	2	15	1
Control	0	2	16	16	2	0

The average achievement-ages and grades for the two groups of *Ss* on the selection-tests are given in Table II.[4] The statistical significance of the differences between the reading-ages of the two groups indicates that the groups were definitely of different samples in the reading processes measured.[5] The average arithmetic-ages and quotients for both groups were within an average achievement-range. The significance of the difference in arithmetic-scores may be higher than the true difference in arithmetical ability because the majority of the control *Ss*, as compared with very few of the experimental ones, had been introduced to division in the fourth grade and because some confusion in carrying out the printed instructions of the arithmetic tests occurred in the experimental group despite the experimenter's attempt to avoid this difficulty.

Table III shows the chronological ages, mental ages, and intelligence quotients obtained on a corrected scoring of the Stanford Binet Intelligence Test for the paired *Ss*. In order to arrive at a rating of intelligence which would accurately represent the ability of the experimental *Ss* who are penalized on certain test items by the possession of reading handicaps, a correction was applied by omitting from a

[4] In order to compute average achievement-ages for the reading handicap group, the procedure of assigning an age-score of 6 yr. to *Ss* who did not score at all on the tests was used since the average chronological age for school entrance, and hence for practically no achievement at all, was six years in this community.

[5] Student's correction was utilized in computing the standard errors of the means. A critical ratio of 2.89 was considered significant. It is Fisher's value of t for 17 degrees of freedom, or n-1 in this study.

revised scoring of the Binet those items which such *Ss*, as opposed to control *Ss*, have been found to fail rather consistently.[6] These items include reading and report, dissected sentences, vocabulary, designs, and repetition of digits backwards. The procedure followed was that of giving the complete Binet and scoring it in the usual manner. Then the correction in scoring was applied for both groups of *Ss* by omitting the above enumerated items up through the last age-level at which three

TABLE II

MEAN ACHIEVEMENT AGES, GRADES, AND QUOTIENTS OBTAINED BY THE EXPERIMENTAL AND CONTROL GROUPS ON TESTS FOR SELECTION

Tests	Groups of Ss	Experimental group			Control group			D/σ_d
		age	grade	quotient	age	grade	quotient	
Reading:								
Stanford Achievement Paragraph Meaning*	M	6.74	1.98	72.11	9.59	3.79	102.94	14.62
	σm	.15	.12	1.59	.13	.10	1.75	
Gates Pronunciation	M	6.75	1.33	72.22	8.97	3.27	95.67	10.83
	σm	.03	.03	.76	.20	.19	2.46	
Haggerty Sigma I	M	7.19		76.56	9.64		103.39	10.38
	σm	.19		1.95	.15		1.72	
Arithmetic:								
Stanford Achievement Computation†	M	8.94	3.30	95.61	9.78	3.96	105.11	4.86
	σm	.14	.09	1.36	.11	.09	1.25	

* Based on the average of scores on 3 forms (V, X, Y) of the test.
† Based on the average of scores on 2 forms (V, X) of the test.

or more items were passed. The number of months represented at each age-level were redistributed among the items remaining after the omission of the penalizing tests. This procedure proved to leave the average rating made by the control group on the uncorrected form almost unaltered. The uncorrected Binet yielded an average intelligence rating for the experimental group which was 3.61 IQ points lower than that of the control group. The difference was reduced to 1.05 IQ points on the corrected scale.[7]

Apparatus. A falling-screen exposure-apparatus, modeled after the Dodge falling-plate apparatus,[8] was used for the presentation of the visual material (Fig. 1). The principle of the apparatus is that of exposing stationary material when a window

[6] L. S. Wolfe and C. C. Miles (unpublished study on the influence of reading disability on the Stanford Binet intelligence rating).

[7] Donald Durrell (The effect of special disability in reading on performance on the Stanford Revision of the Binet-Simon tests, unpublished M.A. Thesis, University of Iowa, 1927) found an average increase of 4.75 IQ-points for *Ss* with reading disability of much wider C.A.- and M.A.-ranges by using a somewhat different correctional scheme on the Binet scores.

[8] Raymond Dodge and T. S. Cline, The angle-velosity of eye movement, *Psychol. Rev.*, 8, 1901, 145-147.

in the falling-screen coincides with one in a front-screen behind which is the printed material. The apparatus was designed with several considerations in view: it had to be portable, adjustable in height, and adjustable in speed of fall so that it could be used for both slow and fast exposures. When the apparatus was to be used for slow exposures, the falling-screen was attached to a piston which moved in an oil filled cylinder. When the oil control was not attached, the instrument could be used for rapid exposures.

TABLE III

CHRONOLOGICAL AGES AND CORRECTED STANFORD BINET MENTAL AGES AND INTELLIGENCE QUOTIENTS FOR EVERY S IN THE EXPERIMENTAL AND THE CONTROL GROUPS

(The Ss of the two groups which were paired are represented by the same numbers.)

Subject pairs	C.A.*		Stanford Binet corrected			
			experimental		control	
	experimental	control	M.A.	IQ	M.A.	IQ
1	9-3	9-1	9-10	106	10-0	110
2	9-5	9-5	10-0	106	10-2	108
3	8-11	8-11	9-4	105	9-4	105
4	9-8	9-7	10-4	107	10-3	107
5	9-3	9-3	9-8	105	10-6	114
6	9-10	9-10	10-6	107	9-7	97
7	9-1	9-1	9-9	107	9-4	103
8	9-4	9-4	9-4	101	9-8	104
9	9-5	9-7	9-2	97	9-1	95
10	8-11	8-10	8-9	98	8-10	100
11	9-3	9-3	8-7	93	8-11	96
12	9-0	9-0	8-8	96	9-0	100
13	9-3	9-1	8-7	93	8-9	96
14	9-11	9-9	8-10	89	9-0	92
15	9-5	9-5	9-7	102	10-0	106
16	9-11	9-10	10-4	104	9-11	101
17	9-2	9-2	9-0	98	9-4	102
18	9-5	9-5	10-1	107	9-10	104
Mean	9.36	9.32	9.47	101.17	9.53	102.22
σ_m	.07	.07	.151	1.35	.127	1.36
D/σ_d	.56			.30		

* Months were expressed in tenths of years in order to describe the groups in convenient units.

Both the supporting and the falling-screen frames were constructed of brass. The former measured 72 cm. by 31 cm. over all; the sides were 1 cm. square with a groove of 3 mm. in which the guides moved. The cross piece measured 2 cm. in width; the bottom piece, 2.5 cm. by 2.5 cm.

The falling-frame measured 28 cm. by 41 cm. over all; the width of the pieces was 3 cm. Attached to the falling-frame were four hard rubber lugs which extended 3.5 mm. beyond the frame and fitted into the grooved guides of the supporting-frame. The falling-frame was pulled up by means of a cord and a brass pulley attached to the center top of the supporting-frame. On the back of the falling-frame was a 1 cm. extension into which could be placed a bristol board screen containing an exposure window. A 31 cm. by 18 cm. aluminum flap for holding the reading-material was attached to the center back of the supporting-frame by two flat springs.

The oil cylinder, measuring 50 cm. in length, was clamped into the bottom of the supporting-frame. The plunger of the oil cylinder was secured to the center bottom

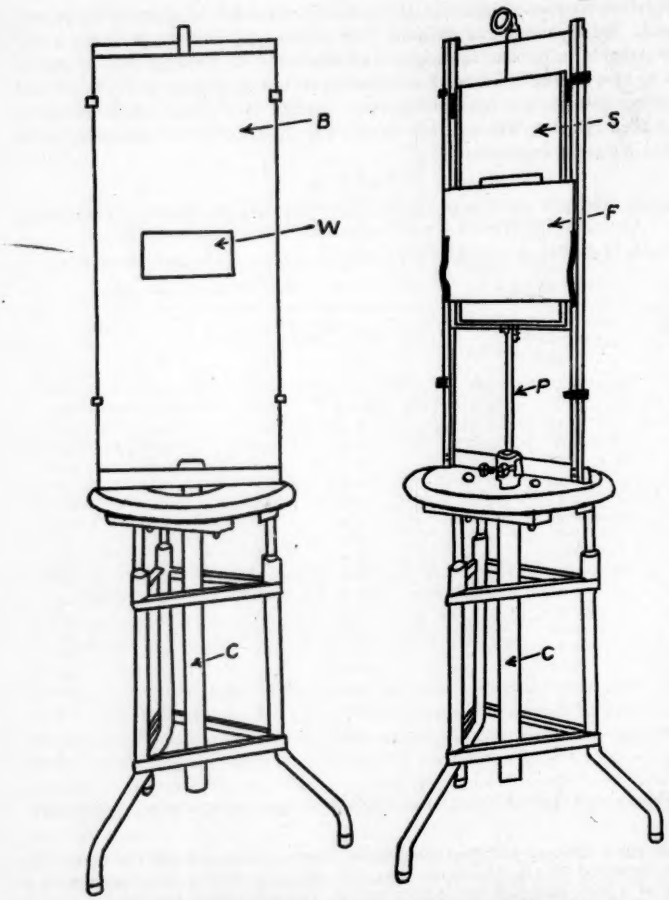

FIG. 1. FRONT AND BACK VIEW OF EXPOSURE-APPARATUS
B, bristol board front piece; W, exposure window; C, oil-filled cylinder; P, piston; F, flap for holding exposure material; S, falling-screen.

of the falling-screen by a screw attachment. Two screws determined the rate of flow of the oil and, consequently, the speed of the fall. The cylinder was filled with kerosene, the viscosity of which allows the fluid to be little affected by temperature changes.

The front piece of the apparatus, containing an 8 cm. by 15 cm. window, was dull neutral gray bristol board; it was fastened to the supporting-frame of the apparatus by four small brass clamps. Dull finish, white bristol board was used for the

screen which carried the window in the falling-frame. Since the distance between the moving window and the material exposed was only 6.3 mm., the same as between the moving window and the front window, no detectable shadows resulted.

The apparatus was mounted on a stable Ever-Hold Automatic Adjustable Stool with three iron legs. The oil cylinder of the apparatus extended through the top of the stool and was held by a brass collar fitted with finger set screws. Two means of adjusting the apparatus so that the exposure window was on a level with *S*'s eyes were possible: adjustment of the height of the stool and of the height of the cylinder in the stool.

The apparatus was used only in well lighted rooms. It was placed facing the source of light and one meter from *S*. This placement proved satisfactory in that shadows on the reading material were not detectable.

Two kinds of exposure-cards, 21 cm. by 30.5 cm., were used: dull finish, white bristol board and glassine, a transparent paper. Lower case letters were used for all the reading material. The different kinds of printing used will be described in connection with the various experiments.

The words used in the experiments were chosen from a word list for primary grades compiled by Gates.[9] Words having the following structural forms were selected: (1) words which could be read meaningfully in a right-left direction (*e.g. was* and *top*) and which, therefore, could give opportunity for *complete reversals* in response; (2) words which could be read by interchanging the letters within the words (*e.g. who* and *bread*), giving opportunity for *partial reversals;* and (3) words which would not be meaningful if reversed unless they were mutilated in some way. Each non-reversible word, chosen as a control for a reversible word, was selected so that it was similar to the reversible one in Gates' rank (indicating the importance and probable frequency of the words in primary texts) and in word-form, *i.e.* in the number of and position within the word of ascending and descending determining letters and of indifferent letters. The final list of 50 words consisted of 15 completely reversible words with 15 controls for them and 10 partially reversible words with 10 controls for them. These 50 words were printed both in a right-left direction and left-right direction, making a total of 100 stimulus-words.

Procedure. Since the study necessitated the presentation of the same list of words a number of times, at least 4 days were allowed to elapse between successive presentations of the list by different exposure-methods in order to prevent the *S*s from developing stereotyped responses. They were told at the beginning of the repeated presentations, "Today you are going to see some more words." The total time required for the presentation of all of the reading material was approximately 6 hr. per *S*. On any one day no *S* worked over one hour and during this time he was given rest-periods between successive 15-min. work-periods.

The responses of the *S*s to the reading material exposed were made orally. This procedure had the advantage of (1) permitting adequate explanation, when necessary, in response to questioning by the examiner, (2) not complicating the responses by motor factors required in writing responses, and (3) eliminating chance elements which might result through any use of a multiple-choice response-method.

Mimeographed word-sheets listing the exposure-material were used by the experimenter in recording *S*s' responses. Whenever the responses were an exact repro-

[9] A. I. Gates, A reading vocabulary for primary grades, *Teach. Coll. Bur. Publ., Col. Univ.*, 1926. 1-23.

duction of the exposed material, either in a left-right or right-left direction, recording consisted in drawing arrows indicating the direction of the response. . l other responses and remarks were given full written records.

The scoring system used was that of counting, as recorded, the frequencies of (1) reversal-errors, (2) all other response-errors combined, (3) "don't know" responses, and (4) correct responses. The responses other than reversals were recorded in order to indicate what happened to the latter in comparison with the former under the various experimental conditions imposed. Reversal-responses for words were classified under two categories: complete reversals and partial reversals. A complete reversal consisted in a right-left directional response either of the entire stimulus-word, *e.g. saw* for *was,* or of the word with a single letter substitution which made possible a meaningful reversal, *e.g. toe* for *eat.* Right-left responses to control-words, including those printed backwards, were also counted as complete reversals. A partial reversal consisted of a change in the order of the letters within a word, *e.g. bread* for *beard.*

I. Effect of Direction of Word-Exposure

The object of Expermient I was to determine the effect of direction of exposure of words upon the frequencies of right-left word-responses and letter-arrangement responses.

Method and procedure. Three different methods of exposure were used: every word (1) was presented as a whole; (2) was presented letter-by-letter in a left-to-right direction; and (3) was presented letter-by-letter in a right-to-left direction. The first method was used in order to obtain responses which could be used as a standard with which to compare reading responses made under the various experimental conditions in the present and subsequent experiments which introduce various controlled factors.

Two different exposure-windows in the falling-screen were used in presenting the material. The window for the whole-word presentations was a horizontal slit, 5 cm. by 15 cm. The one for the letter-by-letter exposures was a diagonal slit, 17 cm. long and 0.7 cm. wide. This slit was the hypotenuse of a right isosceles triangle, the base of which was parallel to the bottom of the screen. In falling, this angular slit exposed only one complete letter at a time. By inverting this screen, both the left-right and right-left letter-by-letter exposures were accomplished. The falling-plate, carrying the screen, was attached to the oil control which was adjusted for a constant, predetermined rate of fall. The exposure-period allowed by the horizontal window for whole-word presentations was 2.17 sec.; that by the diagonal slit for directional presentation was 0.32 sec. per letter.

The order of presentation of the words was arranged so that the highest ranking words (according to Gates' list) were alternated with the lowest ranking ones. Reversible and non-reversible words were arranged in random fashion. The reversed form of any word was not presented at the sitting during which the original form was shown. The entire series of words was presented first by the whole-word exposure-method (hereafter referred to as the standard method). Then, alternations of 10 right-left with 10 left-right letter-by-letter exposures followed until the complete list of words had been presented in both directions. This alternation, it was

hoped, would prevent a directional set from being induced by the experimental procedure or the possibility of *S*'s catching on to the fact that some of the control-words were printed backwards. Half of the *Ss* in each group began this part of the work with the right-left and half with the left-right exposure-method.

Instructions. The instructions were designed to provide as a set the giving of word-responses. For the whole-word exposures the instructions were: "Now you are going to see some words through the window; watch very carefully and tell me the words as quickly as you can. Let us see how many you can get right." Two practice trials were given. For the controlled-direction exposures, *S* was told, "Now you will see some more words, but this time you will see only one letter at a time. Watch carefully and tell me the words as quickly as you can." Two practice trials were given.

In order to determine the correspondence between reversed word-responses and the order of the letters *S* perceived, and also to determine the direction of letter-groups perceived when no words were reported, *S* was asked, "What letters did you see?" The letters of all the correct responses were not requested since it was desired to prevent undue interference with the set to give word-responses. Yet it was desirable to include some correct words in the questioning to allay the suspicion of further questioning on the incorrect word-responses only.

Results. The results are presented in three divisions: (1) word-responses; (2) letter-responses to the words which were reversed; and (3) letter-responses to the words which the *Ss* did not recognize.

(1) Word-responses: (a) Inter-group comparisons. Table IV indicates for each method of exposure the average number of complete and partial reversals and of other responses made by each group of *Ss* for the different word-categories and for all words combined. The number of words reversed by the experimental group exceeded that made by the control in every word-category by amounts producing critical ratios between 1.20 and 6.47. When all the classes of words were combined,[10] the differences between the groups in reversal-errors are all statistically significant, the critical ratios falling between 3.87 and 8.18. The experimental group also made a significantly greater number of other errors and "don't know" responses than did the control group.

All *Ss* of the experimental group reversed some words under each condition of word-exposure. This was not true for the control *Ss*. Two of the experimental *Ss* deviated considerably from the remainder of the group in the number of reversals, one making relatively many and one making relatively few under the three methods of exposure. Three of the control *Ss* made relatively many reversals in comparison with the rest of the controls.

[10] For all combinations, the original data were regrouped and the means and variabilities determined directly therefrom.

TABLE IV

MEAN NUMBER OF RESPONSES TO DIFFERENT TYPES OF WORDS UNDER THREE DIFFERENT METHODS OF SLOW EXPOSURE FOR THE EXPERIMENTAL AND THE CONTROL GROUPS

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Words	Responses	Methods of exposure and groups of Ss													
		Whole-word exposure					Left-right, letter by letter					Right-left, letter by letter				
		exper.		control		D/σ_d	exper.		control		D/σ_d	exper.		control		D/σ_d
		M	σ_m	M	σ_m		M	σ_m	M	σ_m		M	σ_m	M	σ_m	
Completely-reversible (n=30)	CR	2.39	.36	.06	.05	6.47	2.33	.36	.22	.15	5.41	3.50	.91	—	—	3.85
	PR	.22	.10	—	—	2.20	.17	.09	—	—	1.89	.39	.14	—	—	2.79
	AE	4.17	.86	.39	.04	4.40	3.06	.60	.17	.09	4.74	2.78	.54	.72	.23	3.49
	DK	7.00	2.10	.78	.09	2.96	8.28	1.50	.39	.12	5.26	7.67	1.58	.61	.23	4.41
	Cor.	16.44	.99	29.39	.24	12.70	16.17	1.39	29.22	.29	9.19	15.67	1.28	28.67	.15	10.08
Partially-reversible (n=20)	CR	—	—	—	—	—	—	—	—	—	—	.06	.05	—	—	1.20
	PR	1.61	.25	.56	.12	3.75	1.78	.29	.50	.12	4.13	2.00	.36	.78	.13	3.21
	AE	2.67	.58	.17	.09	4.24	2.72	.41	.11	.08	6.21	3.44	.60	.61	.27	4.29
	DK	5.94	1.08	.22	.13	5.25	6.06	1.03	.44	.17	5.40	5.78	1.10	.28	.11	4.95
	Cor.	9.78	.96	19.06	.24	9.37	8.89	1.09	18.83	.24	8.88	9.06	1.01	18.33	.37	8.58
Control, printed left-right (n=25)	CR	.06	.05	—	—	1.20	.17	.09	—	—	1.89	.39	.18	—	—	2.17
	PR	.06	.05	—	—	1.20	—	—	—	—	—	.06	.05	—	—	1.20
	AE	3.89	.66	.06	.05	5.80	2.67	.44	.11	.08	5.69	3.11	.62	.50	.22	3.95
	DK	6.17	1.16	.11	.08	5.22	6.83	1.19	.44	.25	5.24	7.11	1.12	.17	.09	6.20
	Cor.	14.83	1.30	24.83	.16	7.63	15.33	1.21	24.44	.27	7.35	14.06	1.27	24.17	.27	7.78
Control printed right-left (n=25)	CR	5.61	.56	1.06	.53	5.91	5.28	.57	.89	.38	6.36	6.67	.79	1.78	.57	5.04
	PR	.28	.11	—	—	2.55	.17	.09	—	—	1.89	.11	.08	—	—	1.38
	AE	2.06	.45	.94	.30	2.07	3.06	.73	.94	.19	2.83	1.83	.49	1.50	.30	.58
	DK	13.17	1.02	7.61	1.69	2.82	11.72	1.23	7.89	1.50	1.97	12.39	1.09	7.83	1.58	2.83
	Cor.	3.78	.75	14.83	1.73	5.85	5.22	.69	15.28	1.67	5.56	4.17	.63	13.89	1.74	5.25
Total words (n=100)	CR	8.06	.73	1.11	.44	8.18	7.78	.72	1.11	.49	7.67	10.61	1.69	1.78	.57	4.96
	PR	2.17	.35	.56	.12	4.35	2.11	.33	.50	.12	4.60	2.56	.44	.78	.13	3.87
	AE	12.78	2.24	1.56	.43	4.92	11.56	1.52	1.33	.24	6.64	11.16	1.81	3.33	.70	4.04
	DK	32.72	4.20	8.61	1.66	5.33	32.89	4.12	9.17	1.72	5.32	32.94	4.42	8.94	1.78	4.76
	Cor.	44.28	3.76	88.17	2.05	10.25	45.67	3.47	87.89	1.96	10.58	42.94	3.12	85.17	2.45	10.64

(b) Intra-group comparisons. A comparison of the reversals made by the experimental *Ss* under the standard exposure-method with those made by them under the right-left and left-right exposures indicates that the frequency of their reversal-responses was little influenced by the direction in which the words were viewed. Table V reveals these differences in critical ratio units, none of which is statistically significant.[11] Forced viewing of the words in a left-right direction decreased complete and partial reversals (as compared with the standard condition) by only 0.47 and 0.21 critical ratio units, respectively. Right-left viewing of the words increased reversals by 1.18 to 1.88 critical ratio units over those made under the standard condition and left-right exposure-condition.

TABLE V

COMPARISON IN CRITICAL RATIO UNITS OF THE NUMBER OF REVERSALS MADE UNDER THREE DIFFERENT METHODS OF SLOW EXPOSURE FOR THE EXPERIMENTAL AND THE CONTROL GROUPS

Exposure methods	Groups of *Ss* and types of reversals			
	experimental		control	
	complete	partial	complete	partial
Whole and left-right	.47	.21	—	.60
Whole and right-left	1.88	1.28	2.39	2.00
Left-right and right-left	1.83	1.18	1.60	2.00

That controlling direction of word-exposure had only a minor effect upon the almost negligible number of reversals made by the control group under the standard condition is shown in Table V. The differences, expressed in critical ratio units, between the reversals made by this group under the different exposure conditions are not statistically significant.

In short, the effect of direction of word-exposure upon reversals was small in the case of both the experimental and the control groups. Forced viewing of the words in the custom-determined left-right direction or in the opposite direction left the experimental group making about as many reversals and the control group making about as few reversals as each one made when direction of viewing the words was voluntary.

In order to investigate the possibility that reversals might bear a relationship to the *Ss'* relative familiarity with the two forms of a reversible word, a tabulation was made to show how frequently every word used in this experiment was reversed. As an indication of probable familiarity, in terms of frequency of appearance of these words in primary texts,

[11] The usual correction for correlated measures was made in the computation of the critical ratios for intra-group comparisons.

Gates' ranks of the words were used.[12] Admittedly this procedure could yield only an approximation to the frequency with which the *Ss* in the present experiment had been exposed to the various words. It was not feasible, however, to count the number of times every word had appeared in the various books which the *Ss* had actually used in school.

TABLE VI A

RELATION OF GATES' RANKS OF THE WORDS AND OF THE RATIO OF RANKS OF THE TWO FORMS OF EACH WORD TO THE NUMBER OF REVERSALS MADE FOR THESE WORDS UNDER THREE DIFFERENT SLOW EXPOSURE-CONDITIONS BY THE *Ss* OF THE EXPERIMENTAL GROUP

Gates' rank (2) / Gates' rank (1)	Reversible words: complete reversibles	Gates' rank (1)	Exposure manners and no. of reversals made: whole-word	left-right	right-left	Reversible words: complete reversibles	Gates' rank (2)	Exposure manners and no. of reversals made: whole-word	left-right	right-left
1.1	on	8	2	2	4	no	9	5	6	6
1.2	saw	64	8	7	8	was	77			
1.2	pat	730		1	1	tap	901		3	2
1.2	rats	350			1*	star	432		1	
1.5	lap	964			2	pal	1500+	1	1	2
2.4	pin	615				nip	1500+	3	1	4
2.4	pets	377			2, 1*	step	885	1		1
3.2	oh	279	5	7	7	ho	889	7	6	6
4.0	pan	367				nap	1500+	3		2
5.2	nuts	282				stun	1500+			
7.2	but	206			1	tub	1500+	4	2	6
9.8	ten	152			1	net	1500+		1	1
10.9	top	105			1	pot	1141	3	4	3
16.5	stop	67			2*	pots	1141	4*	3*	2, 3*
20.2	not	74	1			ton	1500+			1
	Sums		16	17	31			31	28	39
	Partial reversibles					Partial reversibles				
1.3	den	760			1†	end	970			
1.5	pear	923			1	reap	1500+			
2.0	read	146		1		dear	302		1	
2.1	ant	707	1	2		tan	1500+		1	1
2.4	left	523	2	1	3	felt	1259	4	2	2
3.4	meat	435				team	1500+	1	1	1
6.8	who	62	2	3	3	how	425	5	6	7
7.6	are	34				ear	258		2	2
8.0	bread	158				beard	1259	12	12	14
25.9	eat	50	2		2	tea	1296			
	Sums		7	7	10			22	25	27

* Partial reversals.
† Complete reversals.

[12] The 1500 words in Gates' list are arranged in order of their importance for primary grades, frequency of appearance in representative primary texts being a factor in determining the ranks. The most frequent or important word has a rank of one; the least frequent, a rank of 1500.

The number of reversals made by the experimental group for each of the reversible words is given in Table VI A. The words in these tables are arranged in the ascending order of the ratios of the ranks of the two forms of each word and with the lower ranking, or more frequent, of the two forms appearing in the second column. It will be seen that when the ratios were greater than about three, *i.e.* when the words of a

TABLE VI B

RELATION OF GATES' RANKS OF THE CONTROL WORDS TO THE NUMBER OF REVERSALS MADE UNDER THREE DIFFERENT CONDITIONS OF SLOW EXPOSURE FOR THE EXPERIMENTAL GROUP

Control words	Gates' rank	Exposure and no. reversals			Control words	Exposure and no. reversals		
		whole-word	left-right	right-left		whole-word	left-right	right-left
is	3		1		si	15	12	15
man	28				nam	16	15	16
run	40			1	nur	7	8	10
red	49				der		1	1
she	53				ehs	7, 1*	8	12
into	135				otni	8, 4*	9, 3*	11, 2*
coat	157				taoc	0	1	
puts	168	1*		1*	stup	0		2
hat	185				tah		1	2
too	194				oot	14	12	13
ride	209			1	edir		4	3
drink	252			1	knird	2		3
leg	278				gel	6	5	5
late	340				etal	2	1	2
if	344				fi	14	12	14
set	351				tes			1
pie	390				eip	1		3
sat	394				tas	1		1
must	401				tsum		1	1
ship	469				pihs	2		
pen	525			1	nep	2	2	2
toe	642			1	eot	3		2
year	788				raey	1		2
yet	908				tey			
tag	949		2	2	gat			11
Sums		1	3	8		106	98	123

* Partial reversals; all others were complete reversals.

pair were disparate in rank, the experimental group reversed more of the higher ranking, or less frequent, words of the pairs, *i.e.* presumably the less well-known words of the pairs. The total number of reversals of the higher ranking words of the pairs exceeded those for the lower ranking ones under all conditions of word-exposure. This relationship did not hold, however, for all individual word-pairs. Apparently, two factors were in operation: the more difficult and unfamiliar reversible words

tended to be reported in their more familiar form (*e.g. top* for *pot*), and the majority of reversible words whose ranks would indicate that they are fairly similar in importance in primary texts were reversed in both forms. Hildredth has suggested that when practice has been about equal and simultaneous on both words of a reversal-pair, an "opposites-association bond" is formed and the child is thereby confused.[13] A confusion of this kind may account for the present findings.

The total number of times the experimental *Ss* read every control word in a right-left direction is presented in Table VI B. When the control words were printed backwards, or in a right-left direction, the *Ss* tended to read in a right-left direction more of the words with low Gates ranks than with high ones.

The foregoing word-analysis, preliminary as it is, suggests that words have different "reversal-values" for children of the reading level represented by the experimental *Ss*, one determinant of which may be familiarity with the words as a function of the frequency of their appearance in primary texts.

Reversals made by the control *Ss* were too few to warrant an analysis of them on the basis of word-rank.

(2) *Letter-responses to words reversed.* During preliminary experimentation there were some indications that word-responses did not always agree with the letter-arrangements which *S* apparently observed, or thought he was reporting. Consequently, it seemed desirable as a further step in analyzing the reversal phenomenon to determine the extent of agreement between the reversing of words and the letter-arrangements reported for those words.

Table VII gives the percentage of reversed word-responses whose letter-groups were also given in a right-left direction by the experimental and control groups. Both groups of *Ss* showed a somewhat low correspondence between the reversal of words and the reversal of the letters of those words. The control group showed slightly more agreement than did the experimental, but the differences were statistically insignificant.

The rather low consistency between reversing words and the letters of those words is a result difficult to interpret because of the influence which the set to give words and the actual report of the words preceding the report of letters may have had on the direction of the letter-groups reported. The greater consistency of the control *Ss* may have been due to their resorting to spelling the words which they had reported, an act

[13] Gertrude Hildreth, Reversals in reading and writing, *J. Educ. Psychol.*, 25, 1934, 14.

which they could perform to a more successful extent than could the experimental *Ss* who were known to be backward in spelling as well as in reading achievement.

(3) Letter-responses to the words unknown to S. Since the letter-responses treated in the preceding section were given following word-responses and might have been influenced thereby, the *Ss* were also asked to report what letters they saw after each word-exposure for which they

TABLE VII

NUMBER AND PERCENTAGE OF LETTER-GROUPS REVERSED OUT OF THE TOTAL NUMBER OF WORDS REVERSED

(CR = complete reversal; PR = partial reversal; TR = total number of all reversals.)

Methods of exposure	Reversal responses	Groups of *Ss* and responses: experimental word no.	experimental letter no.	experimental letter %	control word no.	control letter no.	control letter %	D/σ_d
Whole-word	CR	145	59	45	20	8	40	
	PR	39	6	15	10	5	50	1.14
	TR	184	65	35	30	13	43	
Left-right	CR	140	47	34	20	10	50	
	PR	38	9	24	9	4	44	1.75
	TR	178	56	31	29	14	48	
Right-left	CR	191	85	45	32	20	63	
	PR	46	9	20	14	6	43	2.00
	TR	237	94	40	46	26	56	

could give no word-response. The frequencies with which the letter-groups were given in the various directions (right-left, left-right or partial) were tabulated and expressed for each group and every exposure-condition in percentages of the total number of letter-groups given. The results are presented in Table VIII.

(a) Inter-group comparisons. The experimental group exceeded the control group by statistically significant amounts in letter-groups reported in a right-left direction for the standard and right-left exposure-methods. No difference occurred for the left-right exposure condition.

(b) Intra-group comparisons. In the case of the experimental groups, right-left exposure, as compared with the standard exposure, significantly increased the proportion of letter-groups given in a reversed direction (complete and partial reversals combined) while the left-right exposure had no effect at all. A comparison of the proportion of letter-groups reported in a partial and complete right-left direction with the proportion of words reversed reveals that the latter greatly exceed the former. Of the

total number of stimulus-words reported as words, 14%, 13%, and 18% were reversed (all words combined) under the conditions of standard, left-right and right-left exposures, respectively. The corresponding proportions for letter-arrangements were 3%, 3%, and 9%.

The control *Ss* reversed no letter-groups at all when they were exposed by the standard method. Both of the other two methods of exposure resulted in a few reversals, those for the left-right exposure method

TABLE VIII

NUMBER AND PERCENTAGE OF LETTER-GROUPS REVERSED OUT OF THE TOTAL NUMBER OF LETTER-COMBINATIONS

(Cor. = correct responses; CR = complete reversals; PR = partial reversals; TR = total number of all reversals.)

Methods of exposure	Response type	Groups of *Ss*				D/σ_d
		experimental		control		
		no.	%	no.	%	
Whole-word	Cor.	506	97	156	100	3.75
	CR	10	1	—	—	3.33
	PR	7	2	—	—	2.50
	TR	17	3	—	—	3.75
Left-right	Cor.	499	97	142	97	.00
	CR	6	1	2	1	.00
	PR	9	2	3	2	.00
	TR	15	3	5	3	.00
Right-left	Cor.	464	91	133	98	3.89
	CR	32	6	1	1	3.57
	PR	12	3	1	1	1.82
	TR	44	9	2	2	3.89

slightly exceeding those for the right-left method. No differences resulted for this group of *Ss* between the proportion of letter-groups reversed in this experiment and the proportion of word-responses given in a reversed direction; they were both small.

The very few letter-group reversals of the control group in this experiment as compared with the number of reversals following reversed word-responses suggests that giving word-responses (in a reversed direction) prior to letter responses influenced the direction of giving the letters. This influence, as has already been suggested, may have been exerted through the *Ss'* attempts to spell the words which they had given.

II. EFFECT OF ELIMINATION OF REACTIVE EYE MOVEMENTS

This experiment was designed to determine what influence eliminating reactive eye movements would have upon reversals. The reading material

consisted of the same 100 stimulus-words which were used in Experiment I.

Apparatus. The falling-screen of the exposure-apparatus was detached from the oil control and allowed to fall freely upon 4-in. pieces of sponge rubber wrapped around the lower side-pieces of the supporting-frame. A horizontal slit, 8 cm. by 12 cm.,

TABLE IX

MEAN NUMBER OF RESPONSES TO DIFFERENT TYPES OF WORDS EXPOSED UNDER CONDITIONS OF RAPID EXPOSURE

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Words	Responses	Groups of Ss: experimental M	experimental σ_m	control M	control σ_m	D/σ_d
Completely-reversible (n=30)	CR	1.28	.19	.06	.05	6.10
	PR	.11	.08	—	—	1.38
	AE	6.00	.82	2.17	.41	4.16
	DK	11.50	1.32	.44	.17	8.32
	Cor.	11.11	1.27	27.33	.49	11.93
Partially-reversible (n=20)	CR	.06	.05	—	—	1.20
	PR	.78	.25	.61	.14	.81
	AE	4.22	.53	1.39	.49	3.93
	DK	8.33	.97	.28	.13	8.21
	Cor.	6.61	.79	17.72	.53	11.69
Control printed left-right (n=25)	CR	.33	.28	—	—	1.18
	PR	—	—	.06	.05	1.20
	AE	4.67	.57	2.11	.35	3.82
	DK	8.00	1.01	.50	.17	7.35
	Cor.	12.00	1.21	22.33	.45	8.01
Control printed right-left (n=25)	CR	2.61	.35	.94	.28	3.71
	PR	.06	.05	—	—	1.20
	AE	5.11	.98	4.56	.59	.48
	DK	13.89	1.21	5.39	1.10	5.41
	Cor.	3.33	.61	14.11	1.44	6.91
Total (n=100)	CR	4.28	.59	1.00	.28	5.05
	PR	.94	.26	.67	.16	.87
	AE	20.00	2.12	10.22	1.30	3.93
	DK	41.72	3.79	6.67	1.31	8.74
	Cor.	33.06	3.51	81.44	2.57	11.12

formed the window of the falling-plate and gave an exposure of 50 ms. A cross marked upon the center of the exposure screen was fixated by *S* before the exposure of every word. The apparatus was placed one meter from the *S;* the visual angle subtended by the longest word at this distance measured 2°.

Instructions. The instructions were as follows: "You are going to see some words for a very short time; when I say 'ready,' you look at the cross in the window and then you will see a word there. Be sure not to move your eyes until after you have seen the word, or you might not see it. Let us see how many words you can get." Four practice trials were given.

Results. (a) Inter-group comparisons. The responses made by each group of *Ss* and the differences between the groups for the word-categories taken both separately and together are presented in Table IX. For all the word-categories combined, it will be seen that the experimental group significantly exceeded the control group in complete reversals but not in partial reversals. The former group also made a significantly greater number of other errors and 'don't know' responses than did the latter.

(b) Intra-group comparisons. A comparison of the number of reversals made under the conditions of this experiment with those made on the same words during the standard exposure (Experiment I: whole-word, slow exposure) was made for each group of *Ss* to determine the effect of elimination of reactive eye movements with materials of the kind used here. The results in critical ratio units are given in Table X.[14]

TABLE X

INTRA-GROUP COMPARISONS IN CRITICAL RATIO UNITS OF REVERSALS MADE DURING RAPID AND SLOW EXPOSURE OF SAME WORDS

Words	Reversal	Groups of *Ss*	
		experimental	control
Completely-reversible (n=30)	Complete	2.92	.00
	Partial	1.38	.00
Partially-reversible (n=20)	Complete	1.10	.00
	Partial	2.27	.33
Control, printed left-right (n=25)	Complete	.93	.00
	Partial	1.10	1.10
Control, printed right-left (n=25)	Complete	5.08	.22
	Partial	1.69	.00
Total (n=100)	Complete	5.56	.22
	Partial	3.25	.69

In every word-category except one, the experimental group reversed more words during the standard exposure than during the rapid exposure which eliminated reactive eye movements. For the total list of 100 words the differences with respect to both partial and complete reversals were significant.

Although the number of reversals made by the experimental group during the rapid exposure was significantly less than was made by it during slow exposure, it should be noted that the total number of incorrect (including "don't know") responses was not decreased. Examination and comparison of the original records revealed that the reduction in reversals

[14] Intra-group correlations employed in the critical ratio computations ranged from + 0.05 — .163 to + 0.49 ± .124 for all words combined and from — 0.18 ± .158 to + 0.64 ± .097 for the separate word categories.

was due to a compensatory increase in the number of more serious errors or of complete failures probably because of the shortened time for recognition. Thus, elimination of reactive eye movements cannot be said to have decreased reversals in terms of converting them into correct responses.

The direction of the control *Ss*' reading was little affected by elimination of reactive eye movements. The differences were neither consistent in direction nor statistically significant.

III. Effect of Accentuation of Word-Form

In the third experiment an investigation was made of the suggestion that lack of attention to word-form, *i.e.* the visual appearance of a word, produces reversals and that reversal-errors disappear when methods are used which demand a high degree of immediate attention to word-form.

Procedure. Thirty words (10 reversible words printed in both directions, *e.g. was* and *saw;* 10 control words printed in a left-right direction) were selected for use in this experiment from the list of 100 words used in Experiments I and II. The words were printed in outline by a commercial artist. Exploratory experimentation determined the extent to which the separate letters had to be mutilated by outlining to cause 9-yr.-old children considerable difficulty in reading them in isolation.

A check of the difficulty encountered by the *Ss* was made by recording their responses to 10 representative isolated letters printed in outline. The experimental *Ss* failed on the average to identify 34% of the letters; the control group, 32%. The isolated letter 'a,' for example, was almost always called 'c i.' Apparently both groups experienced sufficient difficulty in reading the separate letters to necessitate giving extra attention to word-form in reading the words.

The words were exposed on the falling-screen apparatus. The same exposure-window and exposure-time per word were used here as in Experiment I for the standard exposure.

Results: (a) Inter-group comparisons. The responses to the words in outline print are given in Table XI. It can be seen that the experimental group exceeded the control by statistically significant amounts on both complete and partial reversals.

The differences between the two groups of *Ss* when the responses to the same 30 words in the present experiment were tabulated for the experiments using fast and slow whole-word exposures of the words in Goudy print are also shown in Table XI. The differences between the experimental and control groups were always in the same direction and were statistically significant in every instance except one (partial reversals made to the words in Goudy print, fast exposure).

(b) Intra-group comparisons. Intra-group comparisons of the frequencies of reversals made under the conditions of the present experiment and

those made in the experiments using Goudy print fast and slow exposures, *i.e.* when reactive eye movements were and were not eliminated and when word-form was probably not accentuated, appear in terms of critical ratios

TABLE XI

RESPONSES TO DIFFERENT TYPES OF PRINTING UNDER DIFFERENT EXPOSURE-RATES

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Printing and exposure	Responses	Groups of Ss				D/σ_d
		experimental		control		
		M	σ_m	M	σ_m	
Outline, slow	CR	1.06	.19	.17	.09	4.24
	PR	.61	.12	—	—	5.08
	AE	6.50	.97	1.56	.40	4.70
	DK	5.78	1.43	.22	.13	4.12
	Cor.	16.06	1.28	28.06	.45	8.82
Goudy, slow	CR	.83	.23	—	—	3.61
	PR	.56	.14	—	—	4.00
	AE	5.17	.94	.22	.13	5.21
	DK	5.94	1.29	.17	.09	4.47
	Cor.	17.50	1.59	29.61	.19	7.57
Goudy, rapid	CR	.83	.21	.05	.05	3.55
	PR	.33	.14	.05	.05	1.87
	AE	5.28	.62	1.72	.41	4.81
	DK	9.28	1.01	.33	.14	8.77
	Cor.	14.28	1.19	27.83	.53	10.42

in Table XII. Accentuation of word-form, as produced in this study, had no significant effect on the number of reversals made with the same series of words by either group of *Ss*. The tendency on the part of the group

TABLE XII

COMPARISON IN CRITICAL RATIO UNITS OF REVERSALS MADE UNDER DIFFERENT CONDITIONS OF PRINTING AND EXPOSURE-TIME

Printing and exposure	Reversal	Groups of Ss	
		experimental	control
Goudy, slow and rapid	Complete	.00	1.09
	Partial	1.53	1.00
Goudy, slow and outline, slow	Complete	.96	1.89
	Partial	.23	.00
Goudy, rapid and outline, slow	Complete	1.05	1.20
	Partial	1.56	1.00

handicapped in reading to make reversal-errors and of the control group to refrain from making them was persistent despite the various experimentally controlled factors which have thus far been studied.

IV. Effect of Mirrored Printing

Whether or not mirroring words would alter reading direction was investigated in this part of the study.

Procedure. The 100 words used in the first two experiments were mirrored for the present purpose by inverting laterally sheets of glassine, a transparent paper, upon which the words had been printed, and pasting them on bristol board exposure-cards. The mirroring, of course, reversed not only the lateral direction of the word but also the orientation of the letters themselves within the word. The type and spacings were exactly like those used in the experiments employing ordinary Goudy print. The instructions, exposure-period, and means of exposure were identical with those for the standard method in Experiment I. The *Ss*' responses were recorded in terms of the directions in which the words were read, *i.e.* from right-to-left or from left-to-right.

Results: (a) Inter-group comparisons. Table XIII shows the average number of words of the various categories read in each direction by the experimental and control groups. (The control-words are designated in the table according to the direction in which they could be read meaningfully, *i.e.* in terms of letter order, after they were mirrored. Control words

TABLE XIII

Mean Number of Responses to Mirrored Words

Words	Direction of responses	Groups of *Ss*				D/σ_d
		experimental		control		
		M	σ_m	M	σ_m	
Completely-reversible (n=30)	L-R	8.06	1.00	12.06	.80	3.13
	R-L	3.00	.49	3.89	1.09	.75
Partially-reversible (n=20)	L-R	2.39	.46	3.00	.41	1.00
	R-L	2.28	.42	3.83	1.09	1.32
Control, printed right-left (n=25)	L-R	.83	.35	2.50	.58	2.46
	R-L	6.78	.65	6.39	1.39	.25
Control, printed left-right (n=25)	L-R	9.56	.72	12.50	.85	2.65
	R-L	.39	.23	.89	.36	1.16
Total (n=100)	L-R	20.83	2.44	30.06	1.90	2.99
	R-L	12.44	1.33	14.83	3.34	.67

printed right-left, for example, could be meaningfully read only in a right-left direction unless *S* mutilated the words or read them as nonsense syllables). For all groups of words the control *Ss* exceeded the experimental ones in the number of words reported in a left-right direction, the significance of the differences for 4 out of 5 word-groups being between 2.46 and 3.13 critical ratio-units. The control group also read more words in a right-left direction than did the experimental group except in the

case of control words printed right-left, but none of these differences was statistically significant.

The results of this experiment were also expressed for each group of *Ss* in terms of the proportion of the number of mirrored words read in a left-right or right-left direction to the total number of words read. As Table XIV reveals, the experimental group surpassed the control group in the proportion of words read in a right-left direction for the whole list of words and for two out of the four separate word-categories. Only one of these differences, however, was significant; namely, the one for control words printed in a right-left direction.

TABLE XIV

NUMBER AND PERCENTAGE OF MIRRORED WORDS READ IN DIFFERENT DIRECTIONS

Words	Direction of responses	Groups of *Ss*						D/σ_d
		experimental			control			
		no.	%	σ_p	no.	%	σ_p	
Completely-reversible	L-R	145	73	.031	217	76	.025	.75
	R-L	54	27	.031	70	24	.025	.75
Partially-reversible	L-R	43	51	.055	54	44	.045	.99
	R-L	41	49	.055	69	56	.045	.99
Control, right-left	L-R	15	11	.026	45	28	.036	3.86
	R-L	122	89	.026	115	72	.036	3.86
Control, left-right	L-R	172	96	.015	225	93	.016	1.43
	R-L	7	4	.015	16	7	.016	1.43
Total	L-R	375	63	.020	541	67	.016	1.54
	R-L	224	37	.020	270	33	.016	1.54

(b) Intra-group comparisons. To determine the effect of mirroring words on the reading responses of both groups of *Ss*, a direct comparison of the results of the present experiment (Table XIII) may be made with those obtained in the first experiment (Table IV) when the same words in ordinary printing were exposed under identical conditions (standard or whole-word exposure). Mirroring the words decreased the number of left-right responses for both groups of *Ss* as compared with the number of such responses given to the non-mirrored words. The extent of the decrease was greater for the control than for the experimental group. The right-left responses were increased by mirroring to a significant extent for the former group but to only a small extent for the latter one. Mirroring the words had, thus, far less effect on the responses of the experimental group than on the control group. The right-left responses of the experimental group persisted to about the same extent despite the decrease in the total number of words read. Mirrored printing, while

increasing significantly the number of right-left responses of the control group, decreased the total number of words read by that group to a greater extent than for the experimental group.

In an attempt to throw some light on the meaning of these results a word-analysis on the basis of frequency of response-direction was made. For the experimental group the ratio of the left-right to the right-left reports for the completely reversible words printed so that the letters comprising the words with the lower Gates' ranks were from left-to-right was 6.45; that when the lower ranking word was printed right-left was 1.41. The corresponding figures for the control group were 2.46 and 2.18. Apparently, then, one factor which was operating in the case of the experimental *Ss* was their familiarity with certain words. They may have had a tendency to make words that they knew out of the letter-groups shown and, when these groups were printed right-left, a response classifiable as a reversal occurred. Relative familiarity with the various reversible words apparently was not related to the responses of the control group. Analysis of control words on the basis of the relationship of rank with direction of reading-response was unrevealing of any trends.

A probable explanation of the results of this experiment will be presented in the Discussion; the control *Ss* apparently had a set to correct for the mirroring.

V. Direction of Letter-Reading

This experiment aimed to determine (1) the tendency to reverse lower case single letters and (2) the direction of reading horizontal rows of letters.

Procedure. Pursuant of the first aim 20 letters printed in 24 point bold Goudy type were presented in both a mirrored and an unmirrored fashion.[35] The letters were printed five in a column on bristol board cards and were exposed in succession for 0.65 sec. each by a horizontal slit, 1.5 cm. by 1.5 cm., in the falling-screen. *S* was asked to name every letter as soon as it was exposed.

The second part of the experiment dealt with the direction of reading rows of 'indifferent' lower case letters, *i.e.* letters which had no ascending or descending parts which might cause some to stand out more than others and thereby influence the direction of the reading. Six letters printed in 24 point bold Goudy type with 24 point spacings, both mirrored and unmirrored, were arranged in three different orders.[36] Each grouping of letters was exposed by means of the same window and with the same exposure-period as were used in the first experiment for the standard presentations. *S* was told, "Now you are going to see some letters through the window;

[35] b, a, p, n, s, d, c, z, q, t, n, y, u, o, m, v, w, e, g, f.
[36] avciox; xoicva, civxoa.

name them for me as quickly as you can." No preëxposure fixation-point was used for obvious reasons.

Results. The mean number of lateral reversals of the singly presented unmirrored letters was 1.28 and 0.22 for the experimental and control groups, respectively. The significance of the difference was 3.58. For the same letters mirrored, the mean number read in right-left orientation was 17.22 for the experimental and 16.17 for the control group with a critical ratio of 1.84.

No significant differences were obtained between the two groups in the frequency of reading rows of letters from right to left. When the letters were unmirrored, the mean number of rows read in a right-left direction was 0.11 for each group of *Ss*. The experimental group read on the average 0.50 rows of mirrored letters from right to left; the control group, 0.39. The resulting critical ratio was 0.33.

VI. Reproduction of Designs

That individuals having reading disabilities may be inferior in form memory has been suggested by some experimenters. In order to compare the *Ss* in this study on form memory, especially on direction of reproducing forms, geometric designs unfamiliar to the *Ss* were used.

Procedure. Ten Seashore geometrical designs were presented to the *Ss*.[17] Every design was drawn on a bristol board card and was exposed on the apparatus under the same conditions as obtained in Experiment II (rapid exposure). *S* was given paper and pencil and was instructed to draw what he saw after each exposure.

Results. The experimental group (Table XV) exceeded the control group in the frequency of making the following responses: (1) horizontal inversions; (2) vertical inversions; (3) horizontal and vertical inversions combined in one response; (4) all errors other than inversions;

TABLE XV

Mean Number of Different Types of Reproduction of 10 Seashore Designs

Responses	Groups of *Ss*				D/σ_d
	experimental		control		
	M	σ_m	M	σ_m	
Horizontal inversion	.50	.17	.22	.10	1.44
Vertical inversion	.06	.01	.11	.07	.66
Horizontal & vertical	.33	.14	.06	.05	1.79
All other errors	3.11	.74	2.61	.36	.60
Total	4.00	.58	3.00	.37	1.47

[17] C. E. Seashore, *Elementary Experiments in Psychology*, 1908, 131.

and (5) all errors combined. The control group made more vertical inversions than did the experimental group. None of the differences, however, was statistically significant.

Discussion

The assumption that reversals result from right-to-left visual observation of words was not upheld by the results of this study. In Experiment I, forced viewing of words in a right-left direction produced only a statistically insignificant increase in the number of reversal-responses made by both the experimental and the control groups, whereas forced left-right observation resulted in an insignificant decrease in reversals of the former group and had no effect upon the latter one. Experiment II, which allowed viewing of the whole word while preventing reactive eye movements likewise did not eliminate the experimental group's reversals. The persistence of the experimental group in making relatively many reversals and of the control group in making relatively few despite control of direction of observation and despite elimination of reactive eye movements is evidence, then, that some causal factor or factors other than direction of visual impression as such must be sought to account for reversals.

An explanation of reversals in terms of lack of attention to word-form also appears inadequate. The particular means of insuring increased attention to word-form used in this study (outline printing; Experiment III)[18] produced no material alteration in the experimental group's reversals nor in the control group's relative freedom from such responses.

Experiment IV did not support the expectation that the proportion of right-left reports of the experimental group would be substantially increased by mirroring the words. Only a small increase occurred. The control group, on the one hand, showed a significant increase in right-left reading with the result that this condition of word-exposure was the only one in which the experimental group did not significantly exceed the control in the number of right-left word-responses.

An interpretation of this increase in the right-left responses of the control group and the little change in the responses of the experimental group can only be conjectured at this time. It seems possible that in spite of the number of years of exposure to reading materials the experimental

[18] If the tachistoscopic exposure in Experiment II accentuated word-form at all, it certainly did not do so to the extent that the outline printing of Experiment III did, for the *Ss* could almost invariably report all the letters of the stimuli shown in the former but not in the latter experiment.

Ss were still in a stage of reading in which responses to letters may be made regardless of their orientation or position in the word. Even the severe alteration produced by mirroring the words did not change their proportion of right-left responses. The control group, on the other hand, being in a more advanced stage of reading, presumably should have been responding not only to the letters but also to the position of the letters within the words. With the letter orientation reversed or mirrored, the more mature reaction may have been that of reading the words in a corresponding direction. In other words, it is possible that the control group, recognizing (because of its greater familiarity with the words) that the words were printed backwards, may have had a set to read them accordingly, *i.e.* to correct for the mirroring. Certain observations made during the experiment substantiated this view. The number of control *Ss* who expressed difficulty with the mirrored words exceeded the number of experimental *Ss* who made similar report. Likewise, many more of the former made such remarks as: "these words are backwards," which was of course correct; and "I think thay are up-side-down," which at least indicated recognition of a peculiarity. These individuals attempted to read the words in a right-left direction. One boy asked which way he should read them and, being given no satisfactory reply, stated, "I guess I should read them backwards." He proceeded to do so.

It should be stated that the results of this experiment probably are not directly comparable with those obtained by other investigators who used mirrored material consisting of paragraphs, who worked on *Ss* more alike in reading age, and who took measurements in time rather than errors.[19] The reading material of the present experiment consisted, it will be remembered, in short words only, which were undoubtedly very familiar to the control *Ss*.

The possibility that some learning or stereotyping of responses may have occurred during the repeated presentations of the words should perhaps be mentioned. As has been previously stated, however, at least four days were allowed to elapse between successive presentations in order to minimize such a result. Evidence of learning was not apparent.

In consideration of the general results of the six experiments here reported, the writer wishes to propose that the reading behavior noted in this study may be accounted for in terms of characteristic stages in the process of learning to read, stages which are regularly reached and passed more or less quickly by groups made up of individuals such as

[19] Marion Monroe, Method for diagnosis and treatment of cases of reading disability, *Genet. Psychol. Monog.*, 4, 1928, 335-456.

our control and experimental *Ss*. As habitual methods of attacking words are established, a series of stages is defined. In our control group the school-taught habit of systematically beginning in the custom-determined left-right direction, and thereby always attending to the position of letters within a word as well as to the orientation of the letters, has been more or less adequately set up. The experimental group has not yet reached this stage, with the result that its members frequently respond in terms of partial recognition and association cues which cause errors of various kinds including the so-called reversals. According to this interpretation we would expect to find reversal-errors in an early stage of the learning of average children whose reading progress has followed the usual course at the usual rate.[20] The child with a reading handicap may remain abnormally long at this normal learning stage because of a complex of undesirable characteristics such as those found for the present experimental group, or because of some basic factor or factors which are as a whole or in part responsible for these characteristics.

Descriptions of the present *Ss* with respect to behavioral factors which many workers have investigated in reading disability indicate that as a cross-section of children of average intelligence and from the general school populations retarded with respect to reading, the experimental group of this study may fairly be considered typical.[21] Because of the nature and requirements of this investigation, no actual non-reader incapable of recognizing any words was included.

Summary

As an attack on the nature of the reading behavior involved in making reversals of individual words, measurements were made of the effect of six experimental procedures upon the frequency and consistency of reversal-errors. The first four experiments provided (I) control of direc-

[20] Several studies in the literature on reading, writing, or copying support this point of view although in general they were performed on children representing wide ranges of abilities and were, in the main, cross sectional (L. G. Fildes, *op. cit.*; Gertrude Hildreth, Success of young children in number and letter construction, *Child Develop.*, 3, 1932, 1-14; Reversals in reading and writing, *J. Educ. Psychol.*, 25, 1934, 1-20. Marion Monroe's study of poor readers and normal readers of the same average reading grade should be mentioned in this connection (Methods for diagnosis and treatment of reading disability, *Genet. Psychol. Monog.*, 4, 1928, 335-456.). While both of her groups of *Ss* made reversals and while such errors decreased as the reading age of the groups increased, the poor readers exceeded the normal ones in the frequency of reversals. It seems possible, however, that the reversal frequencies of a control and an experimental group of the same reading age may be found to be fairly comparable in reversals if the groups are selected from school populations and are strictly equated in IQ.

[21] The results will be presented in subsequent papers.

tion of word-exposure, (II) elimination of reactive eye movements, (III) accentuation of word-form, and (IV) mirrored printing of the material. The two further experiments pertained to (V) the frequency of reading single letters in reversed orientation and of reading horizontal rows of letters in a right-left direction, and (VI) the frequency of reproducing certain geometric designs in orientations differing from the stimulus presentations. School children, boys, with and without reading handicaps (experimental and control groups, respectively, with 18 in each group) served as *Ss*.

A falling-screen exposure-apparatus was used for the presentation of the materials. Speed of fall of the screen was controlled by means of a piston, moving in an oil filled cylinder, which could be attached to, or disconnected from, the screen.

The following results were obtained.

(1) Control of direction of word-exposure. The experimental group significantly exceeded the control group in the number of partial and complete reversals under all three methods of word-exposure, *i.e.* whole-word (standard) exposure, left-right letter-by-letter exposure, and right-left letter-by-letter exposure. Forced viewing of the words in a right-left direction produced only a slight and statistically insignificant increase in the number of reversals made by the experimental and control groups, where forced left-right observations resulted in an insignificant decrease in reversal-responses of the former group and had no effect upon the latter one.

An analysis of the individual words with respect to frequencies of reversals indicated that the *Ss*' probable familiarity with the words was related to the frequency with which given words were reversed.

When the *Ss* were asked to report the letters they had seen for the words they reversed or did not know, the following results were obtained. The experimental group significantly exceeded the control in the proportion of letter-groups reversed for the words they did not know. The control *Ss* (apparently because they attempted to spell the words they had given) exceeded the experimental, though not significantly, in the proportion of letter-groups given in a right-left direction following reversed word-responses. Neither group, however, reported as many letter-groups in a right-left direction as it did words.

(2) Elimination of reactive eye movements. Elimination of reactive eye movements left the experimental group still significantly exceeding the control group in the frequency of complete reversals of words. This exposure-method decreased significantly the number of reversals made by the experimental group as compared with the number it made when reactive eye movements were permitted in viewing the same words (standard exposure condition, Experiment I) but had no effect upon the control group. Analysis indicated, however, that many of the words which the experimental group reversed during the standard method of exposure evoked other more serious errors or complete failures during the fast exposures which eliminated reactive eye movements. So elimination of reactive eye movements did not decrease reversals by converting them into correct responses.

(3) Accentuation of word-form. When word-form was accentuated by outline printing, the experimental group again significantly exceeded the control in the number of complete and partial reversals. Intra-group comparisons of the number of reversals of the words in this experiment with those made under the conditions of the foregoing experiments showed that none of the variations in exposure produced significant differences for either group of *Ss*.

(4) Mirrored materials. Mirroring the words had little effect upon the frequency of right-left reports made by the experimental group while it increased those made by the control group as compared with the number it made under the other experimental conditions of this study. That this result is not contradictory to the other results of the study is presented in the Discussion (p. 557); there were certain indication that the control group was set for backwards words and proceeded to attack them accordingly.

(5) Letters. The experimental group significantly exceeded the control group in lateral inversions of separate letters in ordinary print and surpassed it also in the number of mirrored letters read in a right-left direction although this difference was not significant. Practically no difference between the groups occurred in the direction of reading rows of letters.

(6) Geometric designs. The experimental group exceeded the control group in the number of horizontal inversions and double inversions (horizontal and vertical) of geometric designs while the control group surpassed the experimental one in the number of vertical inversions. None of the differences was significant, however.

This study, in short, indicated that reversals of words were remarkably persistent for the *Ss* with reading disability despite the experimental control of factors which have heretofore been suggested as causes of reversals. The control group's maturity of reading response was evident throughout.

It is proposed that the reading behavior displayed in this study may be accounted for in terms of characteristic stages in the process of learning to read. The control group was in the stage in which the habit of systematically attacking words in a left-to-right direction had been fairly well established. The experimental group had not yet reached this stage, with the result that it was responding to partial recognition and association cues which cause various errors including the so-called reversals.

DISCRIMINATION WITHOUT AWARENESS

By JAMES GRIER MILLER, Harvard University

Most of the important concepts of clinical psychology have not yet been found susceptible of laboratory investigation, or at least have not been subjected to it. Nevertheless such undertakings are likely to be most valuable in lessening the gap between the approaches of the clinic and of the laboratory to human behavior. The notion of unconscious behavior is of utmost importance in clinical psychology, and it has from time to time demanded the attention of laboratory investigators. Sparsely distributed throughout the literature, investigations are to be found which have some more-or-less distant bearing on this question.[1]

HISTORY

The experimental work on this problem can be grouped under the following headings: (1) subliminal perception; (2) subliminal learning and conditioned response; (3) the effect of attitude on psychophysical measures; (4) unreported factors in problem solving; (5) liminal measures of attention and distraction; (6) reportable effects of subliminal neural stimulation; (7) the rôle of subliminal stimulation of neurones in inhibition and facilitation, including increase of sensitivity; and (8) the effect of the manner of response on the limen.

It is impossible to consider here the history of the investigations in these several fields. We can, however, summarize a few of the important researches dealing with the first problem listed: *subliminal perception.* In 1863 Suslowa reported his work with the esthesiometer.[2] He found that under electrical stimulation esthesiometer sensitivity was lowered, even when the current was subliminal. Peirce and Jastrow, in 1884, reported experiments with weights less than one jnd. apart.[3] They discovered that judgments of which was heavier were more often right than wrong, though made with no confidence whatsoever.

In 1898, Sidis reported four series of experiments.[4] In the first series he was *S*, using only his right eye, which was amblyopic. Nevertheless he guessed correctly in 71.5% of the cases whether a character presented was letter or figure, so that his results were 21.5% above chance. In the second series, he guessed both whether the object was letter or figure and also what the specific figure was, with successes well above chance. In the third series, he got like results from *S*s with normal vision

* Accepted for publication April 25, 1939.

[1] The author is indebted to Professor Edwin G. Boring for his constant advice and assistance. He presented a preliminary report of this research to the Society of Experimental Psychologists on April 7, 1938. The apparatus used in this study was purchased by a grant-in-aid by the Maria E. McMaster Fund for Psychic Research.

[2] M. Suslowa, Veranderungen der Hautgefüle unter dem Einflusse electrischer Reizung, *Zsch. f. Rationelle Med.*, 18, 1863, 155-160.

[3] C. S. Peirce and J. Jastrow, On small differences in sensations, *Mem. Nat. Acad. Sci.*, 3, 1884, 73-83.

[4] B. Sidis, *The Psychology of Suggestion*, 1898, 1-386.

when the cards bearing the characters were held at such a distance that the *Ss* said they could not differentiate the characters. In the final series, proper names on cards held at a like distance were distinguished better than chance. Sidis stated that "all these experiments tend to prove the presence within us of a secondary subwaking self that perceives things which the primary waking self is unable to get at."[5]

In 1900 Dunlap used a modification of the Müller-Lyer illusion in which the angular lines were subliminal shadows.[6] He found that the illusion was present but slight. In 1904 he reported that *Ss* perceived the interruption of inaudible sounds.[7] Titchener and Pyle repeated Dunlap's experiment with the Müller-Lyer illusion, and reported opposite results in 1907.[8] They added further that they could not find any correlation between the degree of imperceptibility of the arrow heads and the strength of the illusion. Manro and Washburn in 1908 made cards of the Müller-Lyer illusion with inked horizontal lines and lightly pencilled arrows.[9] Following Sidis's procedure, the stimuli were held at such a distance that the pencil lines were not visible. Of ten *Ss*, eight got no illusion, and two got some. The authors made the questionable conclusion that these results confirmed those of Titchener and Pyle. At about the same time Stroh, Shaw, and Washburn[10] carried on modifications of Sidis's experiments. With slightly better control they repeated his letter differentiation experiment, and also whispered the letters subliminally. All the results corroborated Sidis and indicated subliminal perception.

Perky's experiment, in 1910, bears an indirect relation to this problem, and also to the procedure reported in this paper.[11] She was trying to discover whether subjective images and objective presentation could be differentiated. She told the *Ss* to imagine certain objects and to project them on a ground glass screen which was in front of them. At the same time, unknown to them, she projected these images on the screen from behind at a supraliminal intensity (the limen was determined at the end of the experiment), but each *S* thought he saw only subjective images. He was receiving visual stimulation above his normal limen, but he was unaware of it.

In 1913 a student of Hollingworth's carried on an experiment using essentially Manro and Washburn's procedure, and found that 17 out of 20 *Ss* got the illusion.[12] The results were interpreted to indicate wide individual differences of susceptibility to the suggestion of illusion. In 1916 de Laski reported on the basis of experiments that: "It seems certain that subliminal separations of the aesthesiometer-points are discriminable. . . . Discrimination appears to be based not upon length (quantitative)

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[5] Sidis, *op. cit.*, 171.

[6] K. Dunlap, Effect of imperceptible shadows upon the judgment of distance, *Psychol. Rev.*, 7, 1900, 435-453.

[7] Dunlap, Some peculiarities of fluctuating and inaudible sounds, *Psychol. Rev.*, 11, 1904, 308-318.

[8] E. B. Titchener and W. H. Pyle, Effect of imperceptible shadows on the judgment of distance, *Proc. Amer. Phil. Soc.*, 46, 1907, 94-109; also this JOURNAL, 18, 1907, 388.

[9] H. M. Manro and M. F. Washburn, Effect of imperceptible lines on judgment of distance, this JOURNAL, 19, 1908, 242.

[10] M. Stroh, A. M. Shaw and M. F. Washburn, A study in guessing, *ibid.*, 243-245.

[11] C. W. Perky, An experimental study of imagination, *ibid.*, 21, 1910, 422-452.

[12] H. L. Hollingworth, *Advertising and Selling*, 1913, 229-232.

but upon form (qualitative); for the percentage of accuracy does not vary with the difference between the two stimuli. Moreover, we do not find any progressive effect of practice."[13] In 1917 Coover performed a well-controlled experiment based on Sidis's procedure and found that subliminal stimuli exert a positive effect on verbal behavior.[14] Friedline, working on de Laski's general problem, obtained results which she held to show an "extreme delicacy of discrimination" between various cutaneous patterns below the level of the two-point esthesiometer limen.[15]

In 1931 Bressler constructed a Müller-Lyer illusion with Hering gray papers, making the arrows of grays less than one jnd. different from the background.[16] He reported: "We found that the Müller-Lyer illusion is effective in evoking a response even when the arrows are so faint that they are not visible to the observer. . . . The magnitude of this response varies directly with the strength of the stimulus."[17] Presuming that these results are the most recent on the problem of the Müller-Lyer illusion, the score at present may be figured to stand at approximately three to one against Titchener and Pyle, with one tie.

Recently two experiments in many ways like the one reported in this paper have been carried out. Baker in 1937 used both auditory and visual modalities.[18] In the auditory he had the *Ss* distinguish between subliminal dots and dashes; in the visual, between subliminal perpendicularly crossed lines in the 'plus' and 'multiplication-sign' positions. He summarized his results as follows: "(1) Evidence in both the visual and auditory modalities, from the results of all subjects combined, and in the case of each individual subject, gives strong grounds for the conclusion that conscious verbal behavior is influenced by stimuli which are below the conscious judgment threshold. (2) It follows that the physiological thresholds are not coincident with, but are lower than, the thresholds of conscious judgment. (3) Evidence in both the visual and auditory modalities, and from all of the subjects used, gives strong grounds for the conclusion that the influence of the subliminal stimuli is a function of the intensity of the subliminal stimulus."[19]

The latest work of this sort is that of Williams reported last year.[20] Interested in the effect of subliminal cues upon Rhine's extra-sensory perception, he projected from the rear three figures (circle, triangle, square) upon a ground-glass screen. He presented these figures 594 times to 11 unexperienced *Ss* in a prearranged order, showing each figure an equal number of times. They were projected at an intensity below the limen as determined by the method of limits. He then chose the 4 *Ss* with the highest scores for a second experiment. To each of these he represented 8

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[13] E. de Laski, Perceptive forms below the level of the two-point limen, this JOURNAL, 27, 1916, 569-571.

[14] J. E. Coover, *Experiments in Psychical Research at Leland Stanford Junior University*, 1917, 1-641.

[15] C. L. Friedline, Discrimination of two cutaneous patterns below the two-point limen, this JOURNAL, 29, 1918, 400-419.

[16] J. Bressler, Illusion in the case of subliminal visual stimulation, *J. Gen. Psychol.*, 5, 1931, 244-250.

[17] Bressler, *op. cit.*, 250.

[18] L. E. Baker, The influence of subliminal stimuli upon verbal behavior, *J. Exper. Psychol.*, 20, 1937, 84-100.

[19] Baker, *op. cit.*, 98-99.

[20] A. C. Williams, Jr., Perception of subliminal visual stimuli, *J. Psychol.*, 6, 1938, 187-199.

series of 27 figures each at different levels of subliminal intensities, and then 108 control figures at zero illumination. He had the *Ss* report the degree of confidence with which they guessed what the figure was: 1=seen clearly; 2=with doubt but with something seen; 3=a pure guess with nothing seen. In tabulating the results, only figures below the limens which gave rise to '3' judgments were considered. All the results were well above chance frequency, except in the control experiments with no illumination, when the results were at chance frequency. *E* reported that "in no case could any relationship between the number of hits and the intensity of the stimulation be discovered."[21]

Terminology and Problems

The present experiment has the purpose of investigating several questions of subliminal perception. In order to state these problems carefully, three terms must be defined.

The phrase *verbal report* will be used in this paper to indicate the sheer use of the vocal cords to produce a certain recognizable sound. The sounds which will be employed are *circle, square, plus, star* and *wave*, but they might just as well be *one, two, three, four* and *five*, or *ay, eee, eye, oh* and *ooo*. Before any sounds are used, it must be arranged that they are associated with the five proper geometric figures, and this is assured with *Ss* who speak English if the words *circle*, etc., are used. Except for the fact that five sounds produced by the vocal cords are used rather than five keys, 'verbal report' is in all ways the same sort as manual.

Awareness will be indicated by another use of verbal behavior, however. The meanings which the words symbolize and the syntax of the sentences in which the words occur will be considered. When the *S* says "I see a circle" or "I see a wave" or "I see a flicker of light," these will be considered statements of awareness. If he is unwilling to say "I actually see a circle," then he is unaware of a circle.

Discrimination in this experiment will be used to indicate that the *S* is showing characteristic differentiated behavior for one or more of five figures.

The six problems investigated in this experiment are:

(1) Can *Ss* who are entirely unaware that they are taking part in an experiment in visual perception nevertheless perceive?

(2) Under these conditions is there a relation between the strength of a purely subliminal stimulus and the correctness of perception?

(3) Are the limen of awareness and the limen of discrimination equivalent?

(4) Does passing above the limen of awareness have any effect on dis-

[21] Williams, *op. cit.*, 195.

criminatory action? (*I.e.* is discrimination of which the *S* is unaware, merely because he is unaware of it, different from that of which he is aware, as certain clinicians maintain?)

(5) Does the change of set produced by explaining to *S* that he is perceiving alter his limens?

(6) Are there other behavioral results from explaining to *S* that he is perceiving?

Apparatus and Procedure

Apparatus. The arrangement of the experimental rooms is diagrammed in Fig. 1. *S* entered Room 2 through the door at the left. The room was painted black and was illuminated by a 25-watt bulb shaded from the eyes of the *S* as he sat at G, which

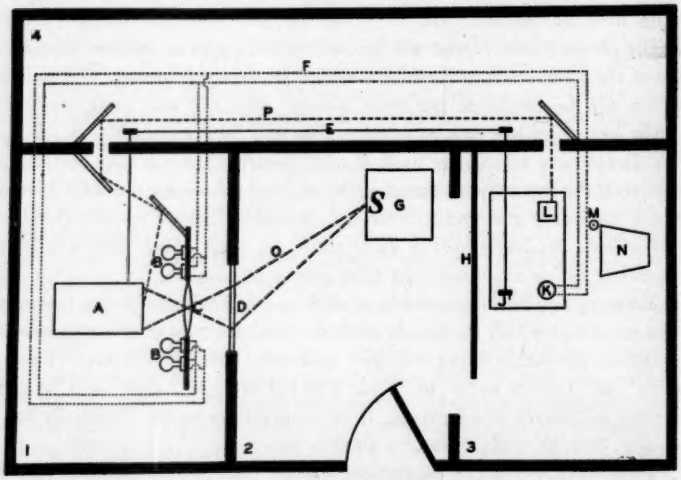

Fig. 1. Diagram of the Apparatus

A, soundproof exposure apparatus; B, electric bulbs; C, lens; D, transparent mirror set in wall; E, pulley system for remote control of exposure apparatus; F, wiring for illumination of cards in exposure apparatus; G, *S*'s chair; H, 4-ft. screen between *E* and *S;* J, control wheel for exposure apparatus; K, variac in illumination circuit; L, end of periscope system; M, signal bell; N, *E*'s chair; O, path of light to *S;* P, path of light through periscope system; 1, 2, 3, 4, separate rooms.

was his position throughout the experiment. Apparently on the wall in front of *S* was an 8 x 10-in. mirror. This, however, was actually not on but in the wall, and was in fact a transparent mirror, which could transmit light from behind although it appeared like a regular mirror from the front. In Room 1, of which the 'naïve' *S* had no knowledge, was a card-exposure apparatus (A) in a soundproof box with a glass window facing *S*. It was made to hold 200 cards, each bearing one of the

five geometric figures of Rhine's test for extra-sensory perception: circle, plus, square, star, wave. These cards were shuffled in chance order in a shuffling machine before they were put into the machine. *E*, who sat at N, could by the wheel (J) on his control board silently expose these cards one by one. *S* had no knowledge of this set-up, for the control board was hidden from him by the screen (H), over which *E* might, however, see him, and the connections between J and A went through an attic (4) of which *S* had no knowledge.

The light reflected from the card exposed in the machine at any one time was caught in the lens (C) and the image was focussed on a milk glass just behind the transparent mirror (D). If the illumination on the card was high enough, its image, approximately 6 by 6 in., was visible on the mirror. The illumination on the card was supplied by a circle of bulbs (B) around the lens. The illumination could be varied by *E* by means of the variac (K) on his control board. Since, during the experiment, the image on the mirror was not strong enough to be visible, a periscope system was set up which reflected the image of each card through Room 4 to the mirror (L) on *E*'s control board. The path of this light is indicated in Fig. 1 by a short-dashed line (P). The long-dashed lines (O) indicate how the light from the card passed to *S* through the mirror. M represents a bell which *E* rang with his foot to signal *S*.

Subjects. Essentially three different experiments were performed. Experiment A employed 5 naïve *Ss*, undergraduates in a beginning course in psychology. They knew nothing of the set-up, and thought, until the experiment was ended, that it had to do with clairvoyance rather than visual perception. Experiment B was identical with Experiment A, except that it was performed on 5 sophisticated *Ss*, one professor of Psychology, 2 graduate students in psychology, and 2 seniors in psychology. They had all had some experience as *Ss* in visual experiments; one had had 30 hours of such work, and another many times that. In Experiment C, 5 *Ss* went through the procedures of both Experiments A and B with certain variations. The *Ss* were 5 students in a beginning course in psychology who performed the experiment as naïve *Ss*, and then repeated it after having had its purposes and the apparatus explained to them.

Experiment A. The following procedure was followed with 5 naïve *Ss*, who will be referred to as *A, B, C, D,* and *F*. Several days before the experiment the *Ss* were shown a pack of the ESP cards and were told about the experiments in clairvoyance developed by Rhine and his co-workers.[22] They were given several clairvoyance tests, and then requested by *E* to take a more rigorous test in the laboratory.

When the *Ss* were taken into Room 2, they were immediately led up to inspect the mirror on the front wall. *E* took ample precaution to see that they had sufficient evidence that it was a real mirror, being certain that they saw their reflections in it and so forth. This, however, was not emphasized so strongly that they had any cause to question that it was a regular mirror. They were then seated in *S*'s chair and given verbal directions by *E* approximately as follows:

"It is a common procedure in clairvoyance experiments for the subject to gaze into a crystal to help him find the answer he wants. This is usually done when the subject tries to picture three-dimensional objects. In this experiment, however, you

[22] C. E. Stuart and J. G. Pratt, *A Handbook for Testing Extra-Sensory Perception,* 1937.

are going to work with the figures on the flat surfaces of ESP cards. I am asking you, therefore, to stare into the flat surface of this mirror. All during the experiment I wish you to stare into the mirror, and not to move your head or your eyes. You may, however, look at the mirror with any part of your eyes that you wish—either fovea or periphery. When your eyes tire, tell me, and we shall rest.

"I shall now go into the room behind you and sit at the table behind the screen. Sitting there I shall expose the cards one by one. When I expose a card I shall ring a signal bell. Please stare at the mirror and try to picture one of the five figures on the mirror as approximately 6 x 6 in. in size, that is about one inch from either side of the mirror and two from the top and bottom. Report the first figure that you picture on the mirror. If you cannot get any cue from the mirror to help you, guess one of the figures, but say that it is a guess.

"Please report also what kind of image you see on the mirror, if you see one; that is, whether it is a memory image, a thought image, an imagination image, an hallucination image, an after-image, or an actual physical image.

"Do not figure that out of every 25 calls you must call 5 of each figure. Though an equal number of each figure will be used, the cards are in a large pack, and it is quite conceivable that chance may turn up a long run of any figure. Please use the following names for the cards: *plus, wave, star, circle, square.*"

E was able by watching *S*'s head at all times to tell if it moved away from the mirror, but of course he had to rely on *S*'s report that he was not turning his eyes away from the mirror. The phrasing of the directions naturally led the *S* to think that the cards were being exposed in the room behind his back, especially since *E* carried a pack of the cards in his hand while giving the directions and took them back to the room with him when he began to experiment. Careful inquiry at the end of the experiment indicated that no other possibility had occurred to any of the *Ss*.

The various sorts of possible images suggested were not defined for the *Ss*, except insofar as they had learned definitions of the terms in their course in psychology. The purpose of this question was mainly to discover whether they were at any time able to report an actual perception 'out there,' or anything which might be an actual perception, and also to assess the degree of their certainty that they did not see anything. This is a behavioral phrasing of Perky's problem of objective and subjective images. The explanation that a large pack of cards was being used was given for the purpose of counteracting a frequent feeling of *Ss* that they are calling too many of one figure. They were instructed to use certain names for the figures because in previous experiments *Ss* were found to use *cross* for *plus*, and sometimes after long use of the word it was found that they were looking for an 'x-shaped' figure, or a figure with the lower vertical arm elongated. It was also found that *Ss* tend to use *wavy lines* for *wave*. This emphasizes the wave over the other figures by using two words for it, and the figure is probably overemphasized in the minds of the *Ss* because it is not a common geometrical figure. Each *S* swore himself to secrecy for the duration of the experiments in order that the naïve *Ss* might remain naïve.

The cards were exposed; the bell was rung immediately after each exposure. At first a long run of the cards was made without any illumination at all to be sure that *S* was not gifted with ESP. There were no significant variations from chance in this procedure.

S was then given 5 series of 25 guesses each, at each of the illuminations represented by 80, 90, 100, and 110 volts on the variac. These voltages were employed

in random order to eliminate possible fatigue effects from the data. The variac readings were converted into illumination readings by means of a photometer 3 yd. from the mirror, and the final results of the experiment are given in terms of illuminations in foot-candles. These voltages were chosen because preliminary experiments with naïve *Ss* showed that they included the significant range.

After the 500 cards had been exposed, *S* was carefully questioned as to whether he had seen any actual physical images in the mirror. His words were written down and he was asked to sign them. Then *S* was allowed to discover for himself the nature of the mirror by raising the illumination to whatever brightness was necessary for him to realize that he was actually seeing a physical image on the mirror-screen. The illumination at which he first realized this fact was recorded. This illumination was taken as his limen of awareness as a naïve *S*. The whole apparatus and purpose of the experiment was then explained to *S*, and his comments were recorded.

Finally *S*'s limen for each of the five figures was determined by the method of limits. For each of the figures the illumination was increased from zero at a constant slow rate until he reported that he saw it. The illumination was then raised to maximum and decreased until he reported no longer seeing it. This procedure was repeated 5 times for each figure with a rest period before each trial. The average of the 5 ascending and the 5 descending trials was taken as his limen for that figure, and the average for all the figures was taken as his limen of awareness in a sophisticated state.

Experiment B. This procedure was repeated for the five 'sophisticated' *Ss* who understood the purpose of the experiment and the apparatus. They will be called *a, b, c, d,* and *f*. They were given 500 trials each, 125 at each of four voltages, but these were taken as 60, 70, 80, and 90 volts instead of the higher voltages, because preliminary experiments with sophisticated *Ss* indicated that these lower voltages included the significant range for them. After each trial they reported whether they actually saw anything on the mirror or not. For them the limen was determined in the same way.

Altogether, in Experiments A and B, 5000 cards were exposed and the comments on each were recorded. *Ss* were at no time told how well they were doing.

Experiment C. Both Experiments A and B were carried through with five originally naïve *Ss* (*1, 2, 3, 4,* and *5*) in this procedure. Each, however, was given only one set of 25 cards, but at each voltage from 60 to 110 in steps of 10 volts. The limens and the point at which the figure was first seen as a real physical image were noted, as above, and then the apparatus and purpose were explained. The procedure was then repeated for them as sophisticated *Ss*. Fifteen hundred more cards were exposed in this experiment.

RESULTS

The data for Experiments A and B are summarized in Table I. In any case where there was a report that an actual physical image was seen (there was no such report in Experiment A), that case was eliminated from the data, and the remaining cases of the run prorated as if for 25.

Since no datum is included in Table I which was given at a time when the *S* was not perfectly certain he did not see an actual physical image on

TABLE I

RESULTS OF EXPERIMENTS A AND B

	S	Number correct out of 125 calls at						Limen of awareness	
		60 v.	70 v.	80 v.	90 v.	100 v.	110 v.	soph. S	naïve S
Experiment A	A	*	*	15	24	27	34	93.2	128
	B	*	*	26	43	69	114	100.6	125
	C	*	*	30	26	33	45	90.8	130
	D	*	*	22	21	26	27	91.3	138
	F	*	*	29	36	49	62	107.3	138
	Av.	*	*	24.4	30.0	40.8	56.4	96.6	132
Experiment	a	20	28	32	29	*	*	125.2	*
	b	22	21	28	32	*	*	105.8	*
	c	28	33	43	52	*	*	97.7	*
	d	31	33	26	39	*	*	92.3	*
	f	30	31	40	70	*	*	108.4	*
	Av.	26.2	29.2	33.8	44.4	*	*	105.9	*

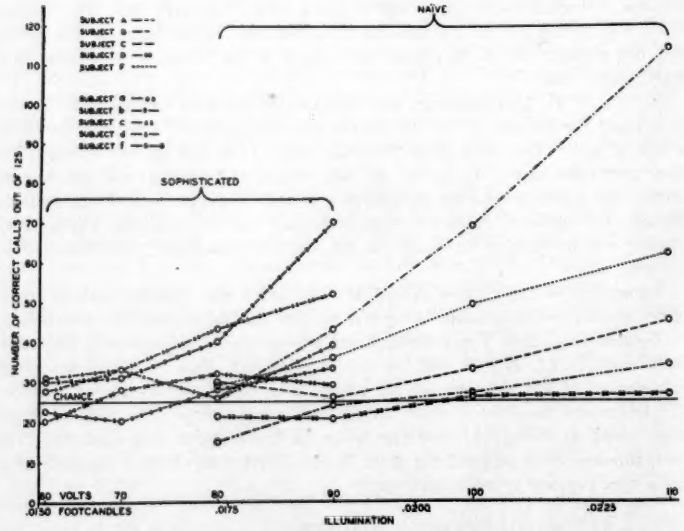

FIG. 2. INDIVIDUAL SCORES OF *Ss* IN EXPERIMENTS A AND B.

the mirror, all cases in the table are definitely below the limen of awareness. The illumination intensities at which these curves are becoming asymptotic to the chance line of 25 out of 125 make it obvious why the illuminations used were chosen. The average curves for both groups of *Ss* are definite

in their trends, with no inversions. The individual scores show 5 inversions totalling 16 points, or an average of 3.2 points. The individual scores for these 10 *Ss* are graphed in Fig. 2.

The data of Experiment C are summarized in Table II.

TABLE II

RESULTS OF EXPERIMENT C

	S	Number correct out of 25 calls at					
		60 v.	70 v.	80 v.	90 v.	100 v.	110 v.
Naïve	1	3	3	8	4	6	15
	2	6	10	5	3	7	13
	3	6	7	5	6	3	6
	4	5	3	4	5	9	10
	5	6	5	6	7	5	8
	Total	26	28	28	25	30	52
Sophisticated	1	8	11	6	12	21	21
	2	3	4	6	6	13	7
	3	4	5	12	6	11	15
	4	6	3	6	18	11	20
	5	4	10	6	6	7	12
	Total	25	33	36	48	63	75

In order that truly naïve *Ss* might be used, who knew nothing of the experiment or of the type of psychological procedure, and who would be willing to accept the explanations concerning clairvoyance without skepticism, the 5 unpracticed *Ss* were chosen. To give the problem as careful a test as possible, however, 5 *Ss* with more experience were also used. It seemed improbable that the same *Ss* could serve both functions, even with a good deal of practice and training, so separate sets of *Ss* were used for Experiments A and B. This procedure might well give rise to the criticisms, however, that an artifact had been introduced, that the limens of two groups would not be comparable, and that their difference would not be significant. In order to make certain of these points, Experiment C was performed as a check, using the same 5 *Ss* in both naïve and sophisticated capacities. The results are graphed in Fig. 3 on the same scale with the average results from Experiments A and B, in order to show that Experiment C is significantly comparable with A and B. This result also shows that no artifact was inserted into the results by the choice of different voltages for the two types of *Ss*. In Experiment C the *Ss* in both naïve and sophisticated capacities were given all voltages from 60 to 110.

It is conceivable that the use of five figures with different limens may have introduced a source of statistical error into the experiment. If it is

true that the circle is the best figure,[23] then it is probable that it can be seen at a lower intensity than, say, the wave. There is later evidence contrary to this position of the Gestalt school,[24] and it is interesting to note from Table III in passing that our data corroborate the first position.

TABLE III

NUMBER OF TIMES THE DIFFERENT FIGURES WERE CORRECTLY REPORTED AT THE DIFFERENT VOLTAGES

Figure	Sophisticated Ss				Naïve Ss				Total
	60 v.	70 v.	80 v.	90 v.	80 v.	90 v.	100 v.	110 v.	
Circle	29	26	44	58	32	40	55	72	356
Plus	28	38	32	58	26	35	49	62	328
Square	31	34	36	43	24	31	40	42	281
Star	29	29	39	41	21	30	37	67	293
Wave	14	19	18	22	19	14	23	39	168
Total	131	146	169	222	122	150	204	282	

Although all the figures were presented an equal number of times, it might have been possible that a preponderant number of the figures with higher limens were presented at the lower voltages and those with lower

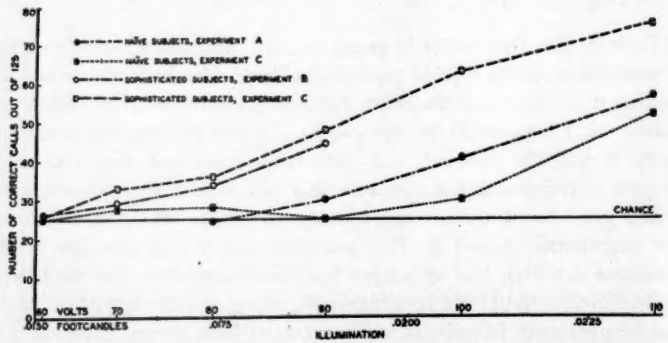
NAÏVE SUBJECTS, EXPERIMENT A
NAÏVE SUBJECTS, EXPERIMENT C
SOPHISTICATED SUBJECTS, EXPERIMENT B
SOPHISTICATED SUBJECTS, EXPERIMENT C
NUMBER OF CORRECT CALLS OUT OF 125
CHANCE
60 VOLTS 70 80 90 100 110
.0150 FOOTCANDLES .0175 .0200 .0225
ILLUMINATION

FIG. 3. AVERAGE SCORES OF NAÏVE *Ss* IN EXPERIMENT A AND OF SOPHISTICATED *Ss* IN EXPERIMENT B, AND TOTAL SCORES OF *Ss* IN EXPERIMENT C.

limens at the higher voltages, and that this was the real cause for the rise of correct calls with voltage. One answer to this criticism is that an actual count of the number of each figure presented to each *S* at each voltage shows no significant variation from chance. Another answer is Table III, which

[23] W. Köhler, The problem of form in perception, *Brit. J. Psychol.*, 14, 1924, 268.

[24] H. Helson and E. V. Fehrer, The rôle of form in perception, this JOURNAL, 44, 1932, 79-102.

shows the data for Experiments A and B fractionated for each figure. It indicates that with only 4 inversions, averaging 3.75 points, the number of correct calls for each figure rose with the voltage, and that in all cases curves for sophisticated *Ss* were well above those for naïve.

The results yielded two other indications that discrimination can go on without awareness. In Table IV are listed the average limens for each

TABLE IV

AVERAGE LIMENS FOR EACH OF THE FIGURES FOR THE SOPHISTICATED *Ss* TOGETHER WITH NUMBER EACH CORRECTLY REPORTED

Figure	Limen (volts)	Number correct
Circle	94.4	356
Plus	98.7	328
Square*	105.9	281
Star	106.7	293
Wave	118.8	168

* The limen for squares was higher than that for stars in the cases of 40% of the *Ss*. The sudden jump to 67 stars at 110 volts in Table III is the cause for the inversion in the last column of Table IV.

of the five figures for the sophisticated *Ss*, and beside them the number of times the figures were called correctly out of all the times presented. The table shows that the higher the limen, the fewer the correct calls—a clear case of visual discrimination, though all *Ss* definitely reported in all cases that nothing was visible in the mirror.

We find in Table V another corroboration of the acceptance of unreported visual cues by counting the number of times images were reported projected (by the naïve) or actually seen (by the sophisticated) for each voltage. As the voltage and illumination rose, the numbers reported rose.

TABLE V

NUMBER OF TIMES IMAGES WERE PROJECTED OR ACTUALLY SEEN FOR EVERY VOLTAGE

Naïve *Ss**		Sophisticated *Ss*	
Volts	Number projected	Volts	Number seen
80	138	60	23
90	147	70	22
100	158	80	31
110	200	90	42

* Among the naïve *Ss*, *D* projected or pictured an image every time, *F*, never; both are omitted because they cancel themselves out.

When deviations from chance are used to indicate the presence of a phenomenon, like discrimination in this case, it is convenient to know what are the probabilities that the results could have come by chance. The chi-square and Pearson's P are conventionally used to indicate this sig-

nificance. In order to make these computations as certain as possible, they were made only for trials that fulfilled two criteria: (1) they must be below the lowest limen found by the method of limits for any *S*; and (2) they must include both naïve and sophisticated *S*s. The runs at 80 and 90 volts fulfilled these criteria. For 80 volts the chi-square value was 18.95. Since these data have 16 degrees of freedom, this gives a P-value of about

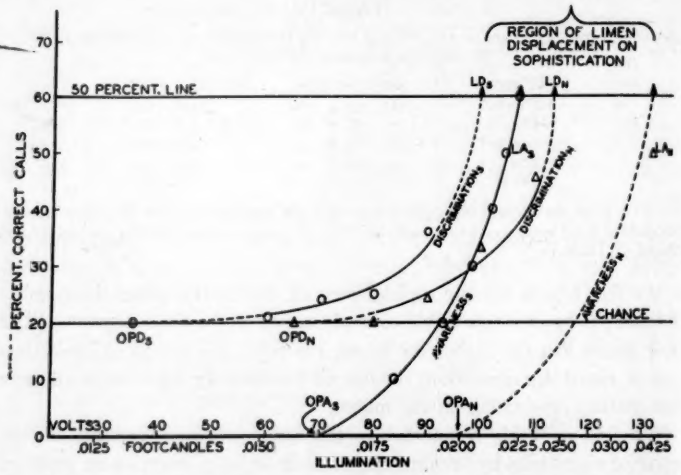

FIG. 4. COMPARATIVE FUNCTIONS OF DISCRIMINATION AND OF AWARENESS FOR SOPHISTICATED AND NAÏVE *S*s.

The solid lines are derived from the data of this experiment; the dotted lines are extrapolations. OPD_S, onset point of discrimination for sophisticated *S*s; OPD_N, onset point of discrimination for naïve *S*s; OPA_S, onset point of awareness for sophisticated *S*s; OPA_N, onset point of awareness for naïve *S*s; LD_S, limen of discrimination for sophisticated *S*s; LD_N, limen of discrimination for naïve *S*s; LA_S, limen of awareness for sophisticated *S*s; LA_N, limen of awareness for naïve *S*s. The "50-percent. line" represents 50% of the distance between the chance-line and 100%.

0.3, meaning that 7 cases out of every random 10 would give results nearer the chance line than these. For 90 volts the chi-square value was 107.68, which gives a P-value of less than 0.001, meaning that more than 999 cases out of every random 1000 would be nearer chance than these data.

One other part of the protocols is of interest. Every one of the naïve *S*s spoke of his surprise on seeing figures he had been unable to perceive, when the experiment was explained at the end of the procedure. *A* said: "I was positively amazed when the set-up was explained to me." *B:* "I was

very surprised when I saw it." *C:* "I was very, very surprised in finding there really was a physical image there." *D:* "I was very much surprised at the set-up." *F:* "I am surprised it [the apparatus] was there." Observers watching the experiment were continually astonished that the naïve *Ss* did not see the images at the higher voltages. An experienced sophisticated *S* (*b*), after discussing the experiment with other psychologists, declared: "I could not get that group to feel as surprised as I about the effect of cues that refuse to be available to introspection. Apparently my attitude is due to the fact that I was a subject."

Discussion

Let us consider the significance of the results in terms of Fig. 4. Here average curves for the sophisticated and for the naïve *Ss* are plotted, showing the results obtained for the functions of discrimination and of awareness. The two discrimination curves are extrapolated upward and downward beyond the points determined by the data of this experiment, as indicated by the dotted lines. These functions are theoretically asymptotic to the 'chance line' of 20% correct calls, because there can be no positive evidence for discrimination except above this line. Actually the functions are tangent to the line at a certain point which can be approximated by extrapolation. This may be called 'the point of onset' of the function. Extrapolation of the curve of discrimination for naïve *Ss* downward on the basis of the data of Experiment C makes it apparent that the point of onset of discrimination for naïve *Ss* (OPD_N) is somewhere between 60 and 70 volts. The data do not indicate the point of onset of discrimination for sophisticated *Ss* (OPD_S), but downward extrapolation indicates that the region would be about 30 or 40 volts.

The locations of liminal points on these discrimination functions are based on the following assumptions. These curves are essentially the lower halves of two ogives such as represent the results of the method of constant stimuli. They are psychometric functions rising with the illumination. The limen is conventionally taken to be the point where the curve intersects the 50%-line. Since, however, chance would in our experiment give 20% correct calls, we must take the half-way point of the distance between 20% and 100%. This is the 60%-line. The data cannot be extrapolated further to show the upper halves of these two ogives.

The data from the determination of the limens of the sophisticated *Ss* by means of the method of limits are the basis of the curve of awareness for sophisticated *Ss*. In constructing this curve, use was made of Urban's

proof[25] that the results of the method of limits are consistent with the results of the method of constant stimuli, and that they can be plotted similarly; that is to say, the average of the least-perceptible value with the greatest-not-perceptible should be the value as often as not perceptible (50%). In this curve the percentage of the total number of trials in the liminal determinations in which the figures were just seen or just not seen at each illumination was added to the sum of the percentages for all the lower illuminations. The ogive thus obtained indicated that the point of onset of awareness for sophisticated *Ss* (OPA_S) was slightly above 70 volts. Only the lower part of this ogive is plotted. The limen of awareness for sophisticated *Ss* (LA_S), then, is at 50% correct calls, halfway between 0% and 100%.

Actually in the calling of the cards, all the cases where *S* reported being aware of an actual physical image were omitted from the results tabulated and the curves plotted. Each datum used in determining the locus of the functions of discrimination in Fig. 4, therefore, was in a sense subjected to a liminal judgment by *S*, who reported that he was unaware of it. As the experiment was conducted, then, no part of the function of awareness for either group of *Ss* lay at a lower voltage than any part of the function of discrimination for that group. The functions of awareness in Fig. 4 are plotted as they are for the averages of groups of sophisticated and naïve *Ss*, and not as they were in the special conditions of the experiment, where the cases were selected in such a manner as to give the problem under consideration the most rigid possible test.

The limen of awareness for naïve *Ss* (LA_N) was found by raising the illumination at the end of the experiment until the naïve *Ss* first saw an actual physical image. The average for all *Ss* was found to be 132 volts. This limen was put at the same level as that for sophisticated *Ss* because it is an analogous limen. Admittedly it cannot be carefully determined by the technique used, for it can be tested only once for every naïve *S*, because as soon as it is reached, *S* is *ipso facto* no longer naïve. Admittedly it is subject to error as it gives only the value for the least-perceptible intensity, and does not average it with the value for the greatest-not-perceptible. Moreover, the *S* may be constrained from speaking because he cannot believe an image would be there. (This point may seem to beg the question from a strictly behavioral point of view, because the limen is taken as the point at which *S* speaks, no matter from what causes. But what if

[25] F. M. Urban, On the method of just perceptible differences, *Psychol. Rev.*, 14, 1904, 244-253.

as soon as he has spoken he says, as did one *S*, "I was aware that a figure was there before I spoke, but I could not believe it until it got brighter"?) Undoubtedly these factors tend to raise the readings for the limen higher than they should be, but the figures for the determinations of the LA_s indicate that the total effect of all these factors is less than 10 volts.

The curve of awareness for naïve *S*s is plotted parallel to that for sophisticated *S*s because it is a similar function, and because the experiment gave no data concerning it, except the point of the limen. To allow for the errors considered in the preceding paragraph, the curve at the 50% locus was made to pass through an illumination slightly lower than that determined by this experiment. The point of onset of awareness for naïve *S*s (OPA_N) is found to be about 95 volts.

Fig. 4 illustrates, therefore, how for each group of *S*s the psychometric function for discrimination lies at lower intensities than the psychometric function for awareness.

The figure also shows that the limens and the psychometric functions of both awareness and discrimination are displaced to lower illuminations by sophistication. This shift was not due merely to a sharpening of attention when the naïve *S*s first became aware that actual physical images were appearing on the mirror. The *S*s knew for what shapes they were looking and what size they would be, yet could not 'see' them. Once they had perceived them, the figures could be reported with less attention.

Why this displacement occurred suddenly with the first statement of awareness of an actual physical image is to the writer the most important question of the whole experiment, because its answer indicates something about the nature of unaware behavior and also hints at an operation which is dependent on the passage of acts from unawareness to awareness. The change of limen in the process of sophistication is sudden and complete. It was undoubtedly a change of set, attitude or motivation, because as soon as the first actual physical image had been reported seen, the limen of awareness went down from 132 to 100.5 (for Experiments A and C), and the whole curve was displaced—*S* was sophisticated. *S* could now see what he could not see before, and he was surprised he had been unable to see it before.

This exclamation of surprise may well be a behavioral event caused by the organism's becoming aware of a stimulus which has been acting upon it. Surprise may be an indication that some part of the environment which has been supposed to be of a certain character turns out not to be. Such surprise has been reported before in the literature. Perky, for instance, noted that, when at the end of the experiment the *S* was asked whether he was

" 'quite sure that he had imagined all these things,' the question almost always aroused surprise, and at times indignation."[26] Again she said that, when the images she used were shown to a number of students in the department of psychology and to some psychologically competent visitors, and they were told that the *Ss* had not known they were objective physical images, "every one of these observers showed great surprise; most of them 'would, of course, take our word' for the facts, but could hardly credit them; a few remained entirely incredulous—'we asked them to believe too much'."[27] Newhall and Sears, speaking of conditioning finger retraction to visual stimuli near the absolute threshold, say that "a sophisticated subject was surprised to learn that he had made three conditioned responses when he thought his finger had not moved during any of the test trials of the session."[28]

SUMMARY

This experiment has shown: *(1)* That *S* can 'discriminate' intensities too low for him to be 'aware' of them.

(2) That there are at least three perceptual conditions: (1) when *S* cannot discriminate the stimulus; (2) when he can discriminate it but is not aware of it; (3) when he can both discriminate and be aware of it. For this reason we cannot equate awareness and discrimination.

(3) That the correctness of the perception of a subliminal stimulus varies with the strength of the stimulus. This rule holds for both naïve and sophisticated *Ss*.

(4) That the range of illumination in which the psychometric function for awareness lies can be affected by attitudinal factors or motivation (in this case, a knowledge of the experimental set-up), but that the psychometric function for discrimination changes correspondingly.

(5) That behavior of surprise brought about by explanations that the *S* is receiving external stimulation is caused by the consequent awareness of discrimination, and may be regarded as an operation indicating awareness of discrimination.

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[26] C. W. Perky, An experimental study of imagination, this JOURNAL, 21, 1910, 431.

[27] Perky, *op. cit.*, 433.

[28] S. M. Newhall and R. R. Sears, Conditioning finger retraction to visual stimuli near the absolute threshold, *Comp. Psychol. Monog.*, 9, 1933, 11.

HIGHER ORDER CONDITIONING

By W. J. BROGDEN, Johns Hopkins University

Pavlov states that conditioned salivary responses based upon food cannot be established beyond the second order (experiments of Frolov), whereas conditioned defense responses based upon shock cannot be established beyond the third order (experiments of Foursikov).[1] Finch and Culler report that conditioned motor responses can be established up to five orders, and presumably to any desired order, by the use of a general energizing stimulus (thoracic shock).[2] These authors attribute Foursikov's inability to obtain conditioning beyond the third order, to failure of maintaining adequate "motivation" in the animal rather than to any inherent difficulty in the problem itself. The thoracic shock which they used (neither an unconditioned stimulus nor a conditioned stimulus for the motor response) provided the incentive or motivation needed to keep the animal responding to the higher order stimuli. They liken the function of the thoracic shock in the dog to the ordinary social incentives in everyday human learning.

Pavlov further states that the second order of the salivary conditioned response is definitely weaker than the first; in defense reactions, the third is weaker than the second, which in turn is weaker than the first. Brogden and Culler,[3] after conditioning animals to four orders by using the same technique as Finch and Culler, measured the relative strength of the orders thus established by the rate of experimental extinction. The first order was found to be about four times as stable as the higher orders. The higher orders (second, third, and fourth) were equal in the resistance to experimental extinction.

The transfer of a response from one stimulus to another indefinitely seems to be a salient characteristic of human voluntary reaction. In view of this and the dissimilarity which was encountered in Pavlov's laboratory between higher order salivary responses and higher order defense responses, it seemed desirable to repeat the work of Finch and Culler and of

* Accepted for publication April 14, 1939. This study was carried out while the author was a fellow of the National Research Council in the Biological Sciences, 1936-37. The aid and encouragement given by Dr. W. H. Gantt is deeply appreciated.

[1] I. P. Pavlov, *Conditioned Reflexes*, 1927, 34.

[2] Glen Finch and Elmer Culler, Higher order conditioning with constant motivation, this JOURNAL, 46, 1934, 596-602.

[3] W. J. Brogden and Elmer Culler, Experimental extinction of higher-order responses, *ibid.*, 47, 1935, 663-669.

Brogden and Culler, using *food* instead of thoracic shock as a constant incentive for the higher orders.

Experimental Procedure

Four stimuli were used: (1) an electric doorbell, B; (2) a column of compressed air directed to the thigh of the left hind limb, A; (3) a 100-watt light, L; (4) a 1000 ~ tone, T. It was planned, in order to offset any inequality of the stimuli, to have every stimulus represent each order in one of four dogs as shown in the following schema:

	Order			
Animal	first	second	third	fourth
No. 1	B	A	L	T
No. 2	A	L	T	B
No. 3	L	T	B	A
No. 4	T	B	A	L

Mongrel dogs, taken from an unselected laboratory stock, were placed in a soundproof, light-shielded test-room where pellets of food were delivered automatically in the food-box at the animal's right. As soon as food was eaten readily, training was started. The first order stimulus (*e.g.* bell) was presented for 2 sec., followed immediately by shock to the left forepaw. Twenty-five such stimulus-combinations were given daily until a score of 92% (23/25) shock-avoiding flexions to the stimulus was reached. At the first of the test-period on the next day, the second order stimulus was given alone to be certain of its indifference. Food was likewise presented to insure its neutrality for flexion. Once these tests were made, the first order and second order stimuli (*e.g.* bell and air) were given in combination (neither shock nor food followed such presentation; shock was given *only* in combination with the first order stimulus). When flexion to this combination reached the 92% level, the second order stimulus (*e.g.* air) was then presented without the first order stimulus (*e.g.* bell), a biscuit of food now being delivered after each flexion.[4] No food was given if flexion failed. Once the second order was established, the third order stimulus and food were tested separately for indifference to flexion before the second and third order stimuli (*e.g.* air and light) were combined. Food was given after each correct response, both to second and third order stimuli alone and in combination. When flexion to the third order stimulus reached the criterion of 92% response, the fourth order stimulus (*e.g.* 1000 ~ tone) and food were tested separately for indifference before training to the fourth order was started. The fourth order was established in the same way as was the third, food following each flexion-response to the stimuli.

If response at any level broke down, training was given at the next test-period on the level just below. For example, suppose an animal were being conditioned to the second order, and response to the second order stimulus began to fail. On the next day, the first and second order stimuli would be presented together. When

[4] All tests were carried out when the animals had fasted for 18-20 hr., in order that the condition of hunger might be kept as constant as possible.

the 92% criterion to the combination was reached, training would then begin again on the second order. However, if with failure of response to the second order stimulus, there followed failure to the first and second order stimuli in combination, retraining to the first order would be instituted. Once the first order was built up to full strength, training would again proceed as outlined above.

In brief, shock was applied only with the first order stimulus. Food was the incentive for the higher orders, being given after each flexion to any higher order stimulus, alone or in combination with another stimulus of the higher order; when the first and second order stimuli were combined *neither* food nor shock was given. The higher order responses were elaborated by combining the stimulus of the next higher order with the stimulus of the order just established, until response occurred to the new stimulus. Both the stimuli and food were tested for indifference for flexion before starting the training to the next higher order. At no time did any of the animals flex the left forelimb when food was given alone. Nor did they respond to any of the stimuli before they were combined with the stimulus of the order just established.

With four orders of conditioning established, each to a response-level of 92%, the relative strength of each order was measured by the rate of experimental extinction. The first order conditioned stimulus was presented without the pedal shock until the criterion of extinction (8% or 2/25) was achieved.[8] The second, third, and fourth orders were successively extinguished to the same level, food being omitted from the test-sequence. After extinction of the four orders, the animals were reconditioned by the original procedure. When retraining was completed, the four orders were once more extinguished; but now the second order was extinguished first, then the third, then the fourth, and lastly the first order. The following scheme shows the training and extinction schedule:

Order of training	Order of extinction			
Original training	1	2	3	4
First retraining	2	3	4	1
Second retraining	3	4	1	2
Third retraining	4	1	2	3

It is clear that each order was extinguished first, second, third, and fourth in each dog.

RESULTS

In the attempt to establish higher order conditioning in four animals (see scheme on page 580), 10 dogs were trained. Of these, only 4 could be conditioned to higher orders. No higher order conditioning was obtained with the stimuli air, light, tone, bell (scheme for Animal No. 2) in 3 animals. Two animals were conditioned to the stimuli light, tone,

[8] The limits 92% and 8% were used in order to reduce the time involved. If one insists on a perfect score at both ends (100% and 0%) with all animals, the time may be increased by weeks; a given dog may undulate around 23/25 or 24/25 for days without giving a single 25/25. Kelley (*Quart. Amer. Statist. Ass.*, 17, 1921, 743) has well shown the value of this range (7% to 93%) for statistical purposes.

TABLE I

RESULTS FROM ANIMALS DEVELOPING HIGHER ORDER CONDITIONED RESPONSES

Column 1, test-period; Column 2, stimulus-combination; Column 3, ratio of correct responses to total trials for test-period, e.g. 24/25 means the animal responded 24 times in 25 trials.

Animal	1	2	3	1	2	3	1	2	3
	1	BA	9/14	24	BA	8/11	42	B	15/15
	2	B	23/25		A	0/3		A	3/10
	3	BA	24/25	25	B	25/25	43	BA	17/17
	4	A	0/2	26	BA	17/18		A	2/8
	5	BA	12/15		A	1/6	44	BA	18/18
	6	B	22/25	27	BA	17/17		A	2/7
	7	B	25/25		A	4/8	45	BA	6/10
	8	BA	25/25	28	BA	16/16		A	0/2
	9	A	0/2		A	2/9	46	B	24/25
	10	BA	25/25	29	BA	17/17	47	BA	18/18
	11	BA	21/21		A	1/8		A	3/7
		A	0/4	30	BA	17/17	48	BA	3/3
	12	BA	16/20		A	1/8		A	9/19
		A	0/5	31	BA	4/10	49	A	6/25
	13	B	22/25		A	0/1	50	A	7/25
No. 1	14	B	25/25	32	B	23/25	51	A	20/25
	15	BA	9/11	33	BA	20/20	52	A	22/25
	16	B	25/25		A	4/5	53	A	24/25
	17	B	5/5	34	BA	2/2	54	A	2/2
		BA	20/20		A	24/25		AL	14/25
	18	BA	19/19	35	A	0/5	55	AL	21/25
		A	0/6	36	BA	16/16	56	AL	24/25
	19	BA	19/19		A	3/9	57	AL	4/4
		A	0/6	37	BA	14/17		L	16/25
	20	BA	13/16		A	2/8	58	L	2/25
		A	0/5	38	BA	6/6	59	AL	13/25
	21	B	24/25		A	11/19	60	AL	23/25
	22	BA	15/15	39	BA	11/11	61	L	23/25
		A	5/10		A	7/14	62	LT	22/25
	23	BA	15/15	40	A	23/25	63	LT	24/25
		A	3/10	41	A	9/23	64	T	25/25
					AL	1/2			
	1	TB	5/11	14	BA	2/2	23	AL	24/24
	2	T	22/25		A	19/25		L	0/1
	3	T	23/25	15	A	23/25	24	AL	25/25
	4	TB	24/25	16	AL	23/25	25	AL	21/21
	5	TB	14/15	17	AL	16/20		L	0/4
		B	10/10		L	0/5	26	AL	21/23
No. 7	6	B	18/25	18	AL	12/16		L	0/2
	7	B	20/25		L	0/7	27	AL	24/24
	8	B	13/25	19	AL	18/20		L	0/1
	9	B	10/25		L	0/5	28	AL	18/22
	10	B	8/25	20	AL	19/25		L	1/3
	11	B	14/25	21	AL	22/23	29	AL	2/2
	12	B	25/25		L	0/2		L	19/23
	13	Bn	23/25	22	AL	23/25	30	L	23/25
	1	LT	9/15	13	LT	4/10	27	TB	4/5
	2	L	18/25		T	1/4		B	11/20
	3	L	23/25	14	L	23/25	28	B	8/25
	4	LT	18/24	15	LT	0/5	29	B	19/25
		T	0/1	16	L	23/25	30	B	13/25
	5	L	19/25	17	LT	17/20	31	B	21/25

TABLE I—*Continued*

Animal	1	2	3	1	2	3	1	2	3
	6	L	24/25		T	3/5	32	B	24/25
	7	LT	23/23	18	LT	5/6	33	BA	13/25
		T	3/3		T	14/19	34	BA	8/25
No. 9	8	LT	11/11	19	T	20/25	35	BA	14/25
		T	10/14	20	T	9/25	36	BA	14/25
	9	LT	1/1	21	T	12/25	37	BA	12/18
		T	23/25	22	T	21/25		A	5/7
	10	TB	0/3	23	T	21/25	38	BA	2/2
	11	LT	7/10	24	T	16/25		A	16/25
		T	1/2	25	T	23/25	39	A	16/25
	12	L	23/25	26	TB	21/23	40	A	23/25
					B	2/2			
	1	LT	0/5	21	LT	6/10	42	BA	17/25
	2	L	18/25		T	8/15	43	BA	13/25
	3	L	21/25	22	LT	2/2	44	BA	12/25
	4	L	24/25		T	21/25	45	BA	8/25
	5	LT	25/25	23	LT	5/5	46	BA	9/25
		T	0/1		T	16/20	47	BA	9/25
	6	LT	2/10	24	LT	2/5	48	BA	5/25
	7	L	19/25		T	1/5	49	BA	12/25
	8	L	24/25	25	L	24/25	50	BA	16/25
	9	LT	13/20	26	LT	2/3	51	BA	14/25
		T	4/5		T	10/22	52	BA	16/25
	10	L	21/25	27	T	8/25	53	BA	20/24
	11	L	24/25	28	T	14/25		A	0/1
	12	LT	11/13	29	T	17/25	54	BA	17/24
No. 10		T	6/12	30	T	18/25		A	0/1
	13	LT	14/15	31	T	9/25	55	BA	22/25
		T	8/10	32	T	14/25	56	BA	19/24
	14	LT	2/10	33	T	24/25		A	0/1
	15	L	23/25	34	TB	23/25	57	BA	19/22
	16	LT	9/10	35	TB	2/3		A	0/3
		T	11/15		B	22/25	58	BA	15/15
	17	LT	4/8	36	B	18/25		A	9/10
		T	11/17	37	B	23/25	59	BA	1/2
	18	LT	2/10	38	BA	17/25		A	15/23
		T	5/10	39	BA	3/25	60	A	17/25
	19	L	24/25	40	BA	19/25	61	A	18/25
	20	LT	8/10	41	BA	16/25	62	A	23/25
		T	9/15						

bell, air; the second animal was trained when the first died before the extinction series was completed.

In higher order conditioning (where a constant incentive is used as motivation for the higher orders), the crux lies between the first and second order. Once the response is established to the second order stimulus, training to the successive higher orders is accomplished with relative ease. To pass from the first to the second order, response must first be evoked by the first and second order stimuli combined. This is the first difficulty, for neither shock nor food can follow the presentation of these stimuli; if shock were to follow, the second order stimulus would then

become another first order stimulus; if food were given, the first order stimulus would lose its discrete position with regard to shock. Two animals failed here. One responded only 3/45 times to the first and second order

TABLE II

DAYS NEEDED FOR EXTINCTION

The letter in parenthesis after each score gives the position in which that order was extinguished. For example; under dog no. 1, row 1, column 2, we read, 4(b). This column is headed "air" which in the original conditioning was the second order stimulus, bell being the first. In the first of the series of extinctions, air was extinguished second (b) after the bell and prior to the light and tone; and 4 days (25 trials each) were needed for extinction.

	Order 1	Order 2	Order 3	Order 4
Dog no. 1	bell	air	light	tone
Extinction 1	10(a)	4(b)	1(c)	1(d)
Extinction 2	12(d)	4(a)	2(b)	1(c)
Extinction 3	2(c)	3(d)	3(a)	1(b)
Extinction 4	2(b)	4(c)	1(d)	3(a)
Total	26	15	7	6
Mean	6.50	3.75	1.75	1.50
S.E.M	2.78	0.25	0.48	0.50
Dog no. 7	tone	bell	air	light
Extinction 1	2(a)	3(b)	2(c)	1(d)
Extinction 2	2(d)	3(a)	1(b)	1(c)
Extinction 3	4(c)	2(d)	2(a)	2(b)
Extinction 4	4(b)	3(c)	1(d)	2(a)
Total	12	11	6	6
Mean	3.00	2.75	1.50	1.50
S.E.M	0.58	0.25	0.28	0.28
Dog no. 9	light	tone	bell	air
Extinction 1	13(a)	6(b)	1(c)	1(d)
Extinction 2	9(d)	8(a)	1(b)	1(c)
Total	22	14	2	2
Mean	11	7	1	1
S.E.M	2.00	1.00	0.00	0.00
Dog no. 10	light	tone	bell	air
Extinction 1	16(a)	6(b)	5(c)	2(d)
Extinction 2	5(d)	2(a)	2(b)	1(c)
Extinction 3	6(c)	4(d)	5(a)	1(b)
Extinction 4	7(b)	2(c)	1(d)	2(a)
Total	34	14	13	6
Mean	8.50	3.50	3.25	1.50
S.E.M	2.53	0.96	1.03	0.28

stimuli in combination during the 14 days during which this training was attempted. The other responded 12/124 times during a training period of 33 days. Neither animal came near reaching 92% response to the first and second order stimuli, the criterion which had to be attained before the second order stimulus could be given alone. Once the

criterion of 92% response to the first and second order stimuli presented together is reached, the second difficulty appears. For the response must now be evoked by the second order stimulus alone and kept alive by the food which is given at the end of each flexion. The difficulty at this stage is twofold. First, there may be failure of response to the second order stimulus. Secondly, the flexion response to the second order stimulus may be replaced by a short-circuiting response to food; at the onset of the stimulus, the animal instead of flexing the forelimb, turns immediately to the food-box in anticipation of food. Three animals failed here. One animal responded to the second order stimulus presented alone only 9/34 times during a training period of 24 days. A second animal responded 9/33 times during a training period of 51 days. The third responded 8/19 times during a training period of 15 days.

Once the second order is built up to the 92% response level, little difficulty is encountered in going on to the third and then the fourth orders. However, one animal (the only one of the three, committed to training with air, light, tone, bell, which showed any evidence of higher order conditioning) attained the criterion of response for the second order after only 6 test-periods, but in the attempt to go on to the third order, the second order response broke down. In 32 days of retraining, the second order response could not again be established, so no further work with this animal was tried.

The results of the four animals in which conditioning to the fourth order was attained are presented in Table I. In column two, B indicates the electric bell-stimulus; A, the air-stimulus; L, the electric light-stimulus; and, T the 1000~ sound-stimulus. The stimulus-pattern BA indicates that bell and air were presented stimultaneously; AL, that the air and light were presented together and so on. The third column shows the ratio of responses to the number of times the given stimulus-pattern was presented during one test-period.

The 92% response-criterion was reached in 10 days by Animal No. 1, in 6 days by Animal No. 7, in 7 days by Animal No. 9, and in 9 days by Animal No. 10 (not given in Table I). Conditioning to the higher orders proceeded as has been described and the difficulties mentioned above surmounted with more or less ease. The following resume of training gives the total number of days on which training to each order occurred, both for the individual animals and for the group as a whole. Whenever two stimuli were combined, that day of training was counted as training for the higher of the two orders represented.

Animal	Order				
	first	second	third	fourth	total
No. 1	20	42	9	3	74
No. 7	8	10	3	15	36
No. 9	14	17	8	8	47
No. 10	18	23	5	25	71
Total	60	92	25	51	228
Mean	15	23	6.25	13.75	57.00
S.E.$_M$	2.65	6.10	1.37	4.43	9.22

It is obvious that the second order was the most difficult to establish, the first next, then the fourth, whereas the third was the most easily established order. This is contrary to the results obtained by Finch and Culler, and by Brogden and Culler, who found a progressive decrease in the number of days of training from the first to the fourth order. On the other hand the total time required to establish higher order conditioning is about the same, whether shock or food be used as incentive for the higher orders. The difference is in the distribution of the time required to establish the individual orders.

Retraining after each extinction took but a short time (1 or 2 days) and was about the same for all orders, first and higher. Summing all retraining for all dogs, we find that 1 day was required in 24 cases, 2 days in 11 cases, 3 days in 3 cases, and 4 days in but 2 cases. This is similar to the results reported by Brogden and Culler.

In Table II is given the time of extinction, in days, for each order. By summing the scores for each extinction, a measure of the strength of the individual order is obtained. Since each order was extinguished in every position (first, second, third and fourth) the possible cumulative effects of earlier extinctions are roughly factored out.

There is a progressive decrease in the time required for extinction as one goes from the first to the higher orders. The total scores for the three dogs (animal No. 9 is omitted since a complete extinction series was not obtained) are as follows:

Animal	Order				
	first	second	third	fourth	total
No. 1	26	15	7	6	54
No. 7	12	11	6	6	35
No. 10	34	14	13	6	67
Total	72	40	26	18	156
Mean	24	13.33	8.66	6	52
S.E.$_M$	6.45	1.20	2.18	0.00	9.29

The first order does not require a significantly greater number of days for extinction than the second, nor the second than the third, nor the third than the fourth, for in no case is the difference between any two

successive orders more than twice its standard error (differences respectively: —10.66, S.E. 6.57; —4.66, S.E. 2.49; —2.66, S.E. 2.18). However, the first order requires a significantly greater number of days for extinction than does the third and fourth (differences resspectively: —15.33 S.E. 6.82; —18.00, S.E. 6.45) and the second order requires a significantly greater number of days for extinction than does the fourth order (difference: —7.33 S.E. 1.20). There is no relationship between the time required to establish any given order and its relative strength as measured by extinction. However, the animal (No. 7) which required the least training to establish the four orders (36 days) also required less time for the extinction series (35 days), whereas Animals No. 1 and No. 10 who required a longer training time, about equal in each case (74 and 72 days respectively), required a longer time for the extinction series, again the time for these two being roughly the same (54 and 67 days respectively).

Discussion

The difficulty in establishing higher order conditioning (with food the incentive for the higher orders) is in direct contradiction to the results reported by Finch and Culler, and by Brogden and Culler. All of the dogs which these experimenters trained (using a thoracic shock as the incentive for the higher orders) became conditioned to the higher orders. Whereas, in this study, only 4 out of 10 dogs could be conditioned to higher orders. This difficulty must be due to the difference in the incentive for the higher orders. It is characteristic of an incentive, classified as a reward, that a given form of behavior must occur before such an incentive can act. On the other hand that type of incentive which can be classified as a punishment acts when there is failure of a given form of behavior. Food can serve as an incentive for the second order response only when the animal has made the appropriate response (forelimb flexion) to the second order stimulus. Even if response does occur to the second order stimulus, food is an incentive only when the animal eats. The thoracic shock is given when flexion to the second order stimulus fails. The shock forces the animal to do something; and since the second order stimulus has been associated with the first order stimulus, that something is forelimb-flexion. The difficulty in training all dogs to higher orders with food the constant incentive is then, in part, due to the action of the incentive, a reward. That this difficulty is not encountered when the thoracic shock is used as an incentive for the higher orders is due to the mode of action of the shock, a punishment. This

cannot alone account for all dogs which failed to condition to higher orders. For two dogs failed to respond to the first and second order stimuli in combination. Their failure at this level has nothing whatsoever to do with the incentive for the higher orders. The addition of the second order stimulus to the first order stimulus was sufficient to cause almost complete failure of response. Further retraining of the first order stimulus combined with shock to the forepaw, did not aid in obtaining response to the first and second order stimuli in combination. These two animals differentiated between the first order stimulus alone, which was followed by pedal shock, and the first and second order stimuli which were not followed by shock. On this basis it can be expected that some dogs will fail to form higher order conditioned responses.

Once established, higher order conditioned responses motivated by food are similar to those where thoracic shock is the incentive for the higher orders. For after extinction, any given order is reëstablished very rapidly (within 1 or 2 days in most cases and no longer than 4 days in any case) whether the incentive be food or shock.

The strength of the higher order responses motivated by food and shock differ markedly. Brogden and Culler reported that the first order required 4 times more days for extinction than any one of the higher orders. The higher orders required about an equal number of days for extinction. In this study where food was the incentive for the higher orders, the first order required 1.8 times as many days as the second, 2.76 times as many days as the third, and 4 times as many days as the fourth. The second required 1.53 times as many days as did the third, and 2.22 times as many days as did the fourth. The third required 1.44 times as many days as did the fourth. Neither in higher order responses with thoracic shock the incentive nor in higher order responses with food the incentive was there any relationship between training-time and resistance to extinction. Nor was there any difference in experimental technique between the two, except that in one case, shock to the left thorax followed the second, third, and fourth order stimuli if there were no flexion; in the other case, food followed flexion to the second, third, and fourth order stimuli. The difference in results must then be due to the kind of incentive used for the higher orders. When thoracic shock is used, the three higher orders are equal in their resistance to extinction, requiring one quarter the number of test-days to reach the extinction criterion that the first order requires. When food is used, the three higher orders all require less time than does

the first; the fourth requires less time than the third, and third less time than the second.

At first glance there seems to be a similarity between the present results and those reported by the Russian workers. For example: Frolov found that the salivary response to the second order stimulus was of less magnitude and of greater latency than was the salivary response to the first order stimulus; and Foursikov found that the flexion response to the third order stimulus was of less intensity and longer latency than that to the second order stimulus, and the response to the second order stimulus was in turn of less magnitude and of greater latency than the response to the first order stimulus. In the present experiment, response to the fourth order stimulus was less resistant to extinction than to the third order stimulus, response to the third less resistant than to the second, and to the second less than to the first. It must be remembered, however, that in the experiment just reported, response to the stimulus of each order reached the 92% level before each extinction. The magnitude and latency of the response (though not measured) was approximately equal for all orders. In terms of the measures used by Frolov and Foursikov the four orders are, therefore, equal in strength. Since neither Frolov nor Foursikov subjected their animals to experimental extinction, no other comparisons can be made.

In view of the difficulties encountered in producing higher order conditioning in the present case, it would seem wise to review the principles involved in the transfer of a given response from one stimulus to another, without there being a direct association between the secondary stimuli and the original stimulus for the response. Higher order conditioning in the Pavlovian sense offers little to our understanding of the transfer of a response from one stimulus to another. Finch and Culler, by introducing an incentive for the higher orders, made it possible to generalize to the functioning of human behavior. They likened the action of the thoracic shock to that of social incentives. A response can be transferred from one stimulus to another indefinitely, if conditions of social approval or disapproval supply "motivation" for the higher order stimuli. Since, however, higher order conditioned responses could be produced in only 4 out of 10 dogs, when food was used instead of shock as incentive for the higher order responses, the above generalization breaks down. For food may be analogous to social approval, and social approval functions as an incentive for humans as often as does social disapproval. The weakness in higher order conditioning as formulated by Finch and Culler lies in the conditions of transfer from first to second order. The first and second order stimuli are given in combination, but neither shock to the forepaw (the unconditioned stimulus and incentive for the first order) nor food or thoracic shock (the incentive for the second and higher orders) follow. There is no in-

centive for response to the first and second order stimuli in combination. This stage in higher order conditioning is difficult to pass and it was here that all of the animals in the present study failed. They either differentiated between the first order stimulus, and the first and second in combination, by responding to the first, and not to the first and second, or failed to respond, or respond frequently enough, for the incentive for the second and higher orders to be effective.

Let us suggest an alternative hypothesis to account for the transfer of a response from one stimulus to another, when the secondary stimuli are not directly associated with the original stimulus for the response. The unconditioned stimulus performs a dual rôle in the conditioning process.[6] It determines the form of the response; *e.g.* shock applied to the forepaw produces flexion. It provides the incentive for response. The conditioned stimulus takes over the first of these functions, that of evoking response. The unconditioned stimulus must, however, be present in its reënforcing capacity in order to prevent experimental extinction. But an indifferent stimulus, neither a conditioned nor unconditioned stimulus, can substitute for the incentive-function of the unconditioned stimulus.[7] If forelimb flexion be conditioned to bell, conditioned flexion will be of the same character if shock be omitted and food be given after each successful response. With this completely "conditioned" response, both evoked and maintained by stimuli previously indifferent, it should be easy to develop a second order response. With the conditions bell→flexion→food, no difficulty should be encountered in proceeding to bell+light→flexion→food and then to light→flexion→food. The third order would go, light+air →flexion→food to air→flexion→food, and so on to the fourth and an indefinite number of higher orders. By this procedure, there would be no time when adequate incentive for response was not maintained. Furthermore, in human behavior, where the number of stimuli and situations available either for eliciting or motivating response would be greatly multiplied, successive substitution for both functions of the original or unconditioned stimulus is possible. Not only can there be a transfer of a response from one stimulus, evoking response, to another, but also a transfer from one stimulus, providing incentive, to another. In other words, the transfer of a response from one elicitory stimulus to another would be brought about and accompanied by the substitution of one incentive-situation for another.

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[6] Finch and Culler, Relation of forgetting to experimental extinction, this JOURNAL, 47, 1935, 656-662.

[7] W. J. Brogden, Unconditioned stimulus-substitution in the conditioning process, *ibid.*, 52, 1939, 46-55.

SUMMARY

(1) Higher order conditioned responses, with food the incentive for the higher orders, could be established in only 4 of 10 animals.

(2) In the original training the second order required the greatest number of trials to reach the response-criterion, the first next, then the fourth, whereas the third was the most easily established order.

(3) The first order was the most resistant to extinction, the second next, the third next, and then the fourth.

(4) Retraining after each extinction took but a short time (1 or 2 days) and was about the same for all orders, first and higher.

(5) There is no relationship between the speed of conditioning and the speed of extinction.

(6) The principles of higher order conditioning are examined and an alternative hypothesis is suggested to account for the means of transfer of a response from one stimulus to another, when there is no direct association between the secondary stimuli and the original or unconditioned stimulus for the response.

RETROACTION AND PROACTION IN SERIAL LEARNING

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It is usual in work on retroactive inhibition to present successively for learning two materials and to examine only the effect of the second upon recall of the first. In the total learning situation, however, it has been demonstrated that a prior learning may affect a later one. The present experiment was undertaken to show that the interaction between the first material and the second may be more significant than is either the proactive or the retroactive effect taken alone.[1]

During the course of the research other problems arose, some of which have been studied in detail. The data show that interpolated material or interpolated activity different from the original learning can cause retroactive inhibition. Comparable proactive effects are, moreover, produced by dissimilar material or activity preceding learning. These results appear to be dependent on the difficulty of the material and on the length of the intervals of presentation.

All the experiments used nonsense material presented serially. A series of two units affords the typical condition of retroactive inhibition: learning the first unit is followed by learning a second. As the series increase in length analysis becomes more complicated, but the intrinsic nature of the problem is unchanged. The chief advantage of serial presentation is the flexibility that it offers by the use of short units, approximately equal in difficulty.

PROCEDURE

Learning material. The learning material was of two types: three consonants presented as a unit, or three lines radiating from one central point. The consonant groups were chosen by writing each of the twenty consonants on a separate slip of paper, arranging them in random order, and dividing them arbitrarily into groups of three. Any combination which *E* believed to be too easy was modified or discarded; such a group might consist of an alphabetical sequence or a familiar set of initials. All the letters were used approximately the same number of times. The groups were presented like nonsense syllables, but reported by being spelled rather than pronounced as a unit. In scoring, each correct letter was given one point credit, whether or not its position in the group was correctly recalled. The maximum score for a group was 3.

* Accepted for publication March 1, 1939.

[1] This experiment was performed in the Psychological Laboratories of Harvard University, where it profited greatly from the suggestions and criticisms of Dr. Dwight W. Chapman.

The lines presented to *O* were 5 mm. long, each pointed in one of the 12 positions of the numerals on a clock. In preparing the material *E* wrote the numbers 1 to 12 on separate pieces of paper and arranged them haphazard in groups of three, then ruled lines in the positions designated by the numbers. Care was taken in composing a series of such units not to present similar visual patterns close to each other; except for this precaution the material was arranged by chance. *O* reproduced the lines in recall by drawing them; he was always instructed to put each line in one of the twelve positions, and not an intermediate one. A line in the correct position was given a score of 1; two lines in the right relation to each other (with an accurately reproduced angle between them) were also scored 1; if all three lines were in the right relation but were misplaced, they were given 2 points instead of the maximum 3. Scores on the lines were consistently lower than on the consonants.

The material was presented on a small memory drum placed on a table in front of *O*. The consonants were typewritten in capital letters; the lines were drawn clearly in pencil. The members of a series were always shown in immediate succession; each was exposed for 1.5 sec. The consonants were reported verbally; the lines were drawn. In an attempt to make the two learning conditions more similar, *O* was required to repeat a word during the entire exposure period; he would say "Friday," for example, at a uniform rate, perhaps three times a second. This response rapidly became automatized in practice trials so that it was not a distraction in any of the learning series; it prevented a verbal reinforcement of the consonants which would necessarily be impossible for the lines.

Order of recall. When a series has been partially learned *O* may recall it in any order he chooses, or he may be required to recall it in the order in which it was presented. The disadvantage of the first alternative is that the time between presentation and recall is not kept constant for all the units; the ones shown last may benefit because they can be reported soon after they have been seen. The disadvantage of the second alternative is that such instructions tend to make *O* emphasize the first part of the list in learning.[2]

An attempt was made to have *O* distribute his effort equally over the entire list and still to retain a constant interval between exposure and recall, by requiring a report on only one member of each series. For a series of five groups of consonants, for example, the five units were presented successively. After the last had been shown, the memory drum continued to revolve; if the first group was to be reported the numeral 1 appeared in the next space. If the second was to be designated there was one blank space, exposed for 1.5 sec. after the fifth group of consonants, and then a 2; for the third two blank spaces followed by a 3, and so on. Six seconds always elapsed between exposure of a unit and the exposure of the number which indicated that it was to be reported. The numbers were arranged in chance order. In the terminology of retroactive inhibition, we may say that there was 6 sec. interpolated activity for position 1, a 6-sec. rest period for position 5, and intermediate ratios of work and rest for the intermediate positions.

The second method of recall was the simplest possible one. As soon as the list had been shown, *O* reported all he could remember in any order he chose. For the series of mixed consonants and lines, he could draw the lines and then report the

[2] G. Raffel, Two determinants of the effect of primacy, this JOURNAL, 48, 1936, 654-657.

consonants, report the consonants and then draw the lines, or do both at once. There was no limitation on the length of time used for recall. With either method, each list was shown only once.

For 8 series, totaling 44 lists, 4 *Os* used one method of recall and 4 *Os* another. Differences among the series followed the same pattern for both methods of recall. For the purposes of this experiment, therefore, the two methods were equally useful; and the experiment was continued with the second and more convenient method.

Tachistoscopic exposure. An exposure period of 1.5 sec., as used here, is much briefer than is customary in studies of retroactive inhibition. It was considered desirable, however, to explore even further in the direction of short-time intervals, using the shortest possible time intervals that permit successive presentation of units. Hylan has shown that, when two or more units are exposed tachistoscopically, they are perceived successively even though they had been shown side by side in the tachistoscope window.[3] When *O* is instructed to read the stimulus-card from left to right, he perceives first the unit on the extreme left, then the second from the left, and so on. This procedure fulfills the requirements of serial presentation, and was therefore employed.

The material used earlier was simplified slightly here: two consonants instead of three, and two lines radiating from a central point were used as units. There were 4 series, each of which contained 10 exposure-cards; on each card were two units placed beside each other. The consonants were printed in gummed letters; the lines were drawn in India ink, and were approximately the same height, width, and blackness as the letters.

M and W were excluded from the consonants because of their heaviness; the remaining eighteen were all used four or five times apiece. The lines were presented as before in the 12 positions of the hands of a clock; separations of 180° were not used. This allowed 5 possible angles of separation; each angle appeared 8 times. No combination of lines was repeated.

The tachistoscope employed in the experiment used an episcotister, and allowed an exposure of 60°. The *Os* were instructed to read the cards from left to right, and to write the letters or lines as soon as they had been exposed. A ready signal was given from 1 to 1.5 sec. before each exposure. The experimental session consisted of an explanation of the procedure; the exposure of 4 practice-cards (each was representative of one of the 4 series of this portion of the experiment); repetition of these cards if *O* requested it; presentation of the 4 series. There were 5 *Os*, making it possible to vary the order of presentation so that each series was presented first, second, third, and fourth.

As in the previous scoring, a consonant reported correctly or a line drawn in the right position was given a score of 1; and if the position of both lines was wrong but the angle of separation was reproduced correctly a score of 1 (half credit) was given. The maximal score for any series was 20 for each position. Since two consonants or one pair of lines had been demonstrated in preliminary work to be within the span of attention, scores much lower than 20 indicate inhibition between the units.

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[3] J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 373-403, 498-533.

Multiplication. In one pair of series, two units of interpolated activity consisted of multiplication instead of learning. In one case 3 groups of consonants and then 2 simple multiplication problems were exposed; for the other series there were 2 multiplications and then 3 groups of consonants. There was the usual 6-sec. interval between exposing a group of consonants and designating it for report, for the *Os* with the first method of recall. No record was kept of the accuracy of the multiplications.

The instructions with this procedure were: "You are going to be shown two multiplications, such as 8 x 9, or 2 x 17, and three groups of consonants. As you see each group of consonants, try to learn it. When you see the multiplication, say the answer aloud. You will not be asked to remember the multiplications, so do not try to learn them. The purpose of this series is to have you do some kind of work other than learning for three seconds; all you are expected to do is to try to multiply. If you cannot get the answer while the problem is being shown, forget about it as soon as the drum turns; as long as you were busy thinking about it when the multiplication was in front of you, you did the right thing. Your answer will not be recorded."

After the practice trials the *Os* had little difficulty in following instructions. They gave every indication of trying to perform the multiplications, but dismissed them readily after the exposure period, with little or none of the tension that might be expected to follow an incompleted task.

Series Used in the Experiment. Six *Os* learned the first 13 series. Those series were constructed as is indicated in the following list.

Series I .....2 groups of consonants
Series II .....3 groups of consonants
Series III .....5 groups of consonants
Series IV. .....10 groups of consonants
Series V .....2 sets of lines
Series VI .....3 sets of lines
Series VII .....5 sets of lines
Series VIII .....3 sets of lines followed by 2 groups of consonants
Series IX .....2 groups of consonants followed by 3 sets of lines
Series X .....2 sets of lines followed by 3 groups of consonants
Series XI .....3 groups of consonants followed by 2 sets of lines
Series XII .....2 multiplications followed by 3 groups of consonants
Series XIII .....3 groups of consonants followed by 2 multiplications

Five *Os* learned the following series, which were exposed on the tachistoscope.

Series XIV .....2 groups of consonants
Series XV .....2 sets of lines
Series XVI .....1 group of consonants followed by 1 set of lines
Series XVII .....1 set of lines followed by 1 group of consonants

Results and Discussion

The data are presented in Tables I and II. When, as in Series I, two groups of consonants are presented successively to *O*, both the first and the second can be reported equally well. A series of this length seems to be within the memory span, as the term refers to the case in which *O* can almost always reproduce material perfectly.

When a series of three groups of consonants is presented, however, the scores

for all the positions are lower than in the previous case. Apparently Series II, containing nine consonants, exceeds the span of memory; only 6.7 are reported as a mean score. Such a sequence may be considered to be composed of a series of two groups, comparable to Series I preceded by a single group of consonants. From this point of view the second and third positions are equivalent to the first and second positions of Series I; the scores obtained might then be predicted to be—,2.8, 2.8. Since the actual scores are—,2.1, 2.5, it is clear that the first group exerts a proactive inhibitory effect upon the second and third. Following the same line of reasoning, the third position shows a pronounced retroactive inhibitory effect upon

TABLE I

SCORES OBTAINED AFTER A SINGLE PRESENTATION OF EACH SERIES ON A MEMORY DRUM

The scores of the positions in which consonants were learned are printed in lightface; those of the lines are printed in boldface.

(Maximum score=3)

Series	Position									
	1	2	3	4	5	6	7	8	9	10
I	2.8	2.8								
II	2.1	2.1	2.5							
III	1.1	1.3	1.7	1.9	2.7					
IV	1.2	1.2	1.2	1.0	1.2	1.4	1.3	1.1	2.1	2.7
V	2.1	2.5								
VI	1.7	1.9	2.3							
VII	1.4	1.7	1.4	1.4	2.3					
VIII	1.4	1.7	1.5	2.5	3.0					
IX	2.7	2.7	1.5	1.6	2.1					
X	1.6	1.6	1.3	2.1	2.6					
XI	1.5	1.9	2.2	1.6	2.0					
XII	—	—	1.9	2.1	2.1					
XIII	1.6	1.6	1.9	—	—					

the first and second. But the analysis is incomplete without consideration of the series as a whole, which shows that the retroactive inhibition is stronger than the proactive, since the score of the last position is higher than that of the first two.

In Series III, fifteen consonants were presented, and the mean number reported was 8.7, although only 6.7 had been recalled when nine consonants were exposed. If the same type of analysis is made of these data as of those of Series II, it will be found that the mean of the last two positions is only slightly lower in Series III than in Series II. This indicates that the proactive inhibition rapidly approaches a maximum as the number of units is increased.

This increase brings about one interesting change: although the mean score of the last positions is relatively constant, the difference in value of the different positions becomes more pronounced. Presented graphically, this appears as an increased tilt of the curves of score *vs.* position; the last position has a greater advantage over the penultimate in a series of five units than in a series of three. The retroactive inhibition is also greater in this series than in the preceding one: the mean of the first two positions is 1.2 when they are followed by three groups of consonants, but it is 2.1 when they are followed by only one consonant group.

In Series IV, ten groups were presented successively, and a levelling off in the retroactive inhibition appears which seems directly opposed to the tilting of the proactive effect: the scores of the first eight positions vary about 1.2, and this variation is clearly within the limits of experimental error. The mean of the last two positions is somewhat higher in this series than in Series II and III, while the mean of the last three positions is only slightly lower than in Series III. Proactive inhibition, then, shows little change with increasing length, but proactive tilt is accentuated; positions 1-3 in Series II scored from 2.1 to 2.5; positions 3-5 in Series III from 1.7 to 2.7; and positions 8-10 in Series IV from 1.1 to 2.7.

The same general tendencies appear in the series of lines, if allowance is made for the fact that the lines were more difficult than the consonants. Difficulty rather than quantity of the material to be learned determines the influence of inhibitory effects, as can be seen in Series V, where two sets of lines were presented. The number of errors was relatively high—an accuracy of only 70% for the first position—so that in this case a series of two lies beyond the memory span. As occurs with the consonants when the length of series exceeds the span, there is a difference in score between the positions. The first is lower than the second, indicating again that retroactive inhibition is stronger than proactive inhibition.

In the series of three sets of lines, the scores are lower; and in Series VII, with five sets of lines, the scores for all positions except the last are lower still. This series seems to show the same pattern of position effects as appeared in Series IV, where there were ten groups of consonants. Assuming the lines to be about twice as difficult, so that one set of lines is equivalent to two groups of consonants, the parallel is marked: the first eight positions of Series IV have approximately the same score, and the ninth and tenth are higher; the first four positions of Series VII are approximately uniform, and the fifth is higher.

According to most theories of retroaction, inhibition does not take place between dissimilar materials. The next four series test this theory: each of them is composed of five units, of which either two or three are consonants, and the other three or two are lines. If the scores are like those where only two or three units were presented, no interaction has occurred; if the scores are lower or show proactive tilt, then there has been inhibition or interaction.

A glance at the data of Table I shows that the second alternative describes the results. When three sets of lines are presented alone, their scores are 1.7, 1.9, 2.3. When three sets of lines are followed by two more sets, the scores are 1.4, 1.7, 1.4. When they are followed by two groups of consonants, the scores are 1.4, 1.7, 1.5—almost identical with the preceding case.

In the same series, Series VIII, the two groups of consonants score 2.5 and 3.0 Two consonant groups presented alone (Series I) score 2.8, 2.8. Thus it seems that the three first units caused a proactive tilt of the two last units; and those two last units caused a retroactive inhibition of the three first ones. This reciprocal effect is present in Series X and XI as well as in Series VIII, although it is not apparent in Series IX, where the two first sets of consonants score almost as high as if they had been presented alone, but lower the scores of the three sets of lines that follow them. These data warrant the conclusion that both proactive and retroactive inhibition can occur between dissimilar learning materials.

Robinson has suggested that besides a similarity of content between the original

and the interpolated materials,[4] there may be a similarity of forms of presentation or of the processes involved. Since this experiment is concerned with serial learning, a certain formal similarity of presentation is necessary. "Processes involved" probably refers to the set of *O;* this factor was varied in the next two series. Instead of being required to learn both the original and the interpolated activity, *O* was instructed to learn three groups of consonants, and to perform multiplications in the other two units of the series.

The data of Series XII and XIII show that multiplication inhibited learning the consonants. Three groups of consonants presented alone scored 2.1, 2.1, 2.5; three such groups presented after multiplications scored 1.9, 2.1, 2.1; when multiplication followed the three groups, the scores were depressed to 1.6, 1.6, 1.9. The proactive effect is as usual less pronounced than the retroactive; but in both cases there is evidence of inhibition.

In most experiments on retroaction, no inhibition has been found when dissimilar materials were learned. In the series just described, retroaction has been demonstrated not only with dissimilar learning material but even with different interpolated activity. Much shorter time intervals than are customary were used here; this may have been the cause of the increased susceptibility of the material to inhibition. In that case, even shorter time intervals may result in even more pronounced inhibitory effects between dissimilar materials. This possibility was tested in the next four series, with tachistoscopic presentation. The results are presented in Table II.

Two groups of consonants were shown in Series XIV, and two sets of lines in Series XV. In both series, the first unit was reported more accurately than the second by each *O;* this shows that the material was read from left to right, according to the instructions. The scores of the consonants were consistently higher than the scores of the lines; we may infer that the lines were more difficult.

TABLE II

SCORES OBTAINED AFTER A SINGLE PRESENTATION OF EACH SERIES ON A TACHISTOSCOPE

Thes cores of the positions in which consonants were learned are printed in lightface; those of the lines are printed in boldface.
(Maximum score = 20)

Series	Position	
	1	2
XIV	18.8	17.8
XV	14.4	10.8
XVI	18.4	11.4
XVII	14.4	16.4

Series XVI and XVII were mixed, composed of one consonant group and one set of lines. The consonant scores are slightly lower, for each position, than they were when only consonants were shown; the scores of the lines are the same for the first position and slightly higher for the second. This implies that with tachistoscopic exposure, inhibition is determined by the difficulty rather than the similarity of the

[4] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 128), 1-57.

material: lines have a stronger inhibitory effect than consonants, whether the material they inhibit be consonants or other lines.

THEORY

A large group of diversified experiments deals with the relation between general body processes or affective and emotional conditions, on the one hand, and retroactive inhibition, on the other.[5] This work shows that pleasant odors and sleep after learning facilitate report; unpleasant odors, unpleasant emotional conditions and activity tend to inhibit it. Such conditions probably have on *O* a general effect, which would influence most of his activities as well as being detrimental to recall. The experiments show, therefore, that the retention of material that has been learned is not wholly independent of the rest of the body processes. On the other hand, most of the experiments which relate inhibitory effects to similarity indicate a high degree of specificity for learned material. Apparently memory of such material is affected somewhat by the general activity of *O*, but is at the same time differentiated from other memories.

The results of the experiments reported here indicate that the differentiation does not take place instantaneously at the exposure of the learning material. With a very brief interval between perception of the units, as in tachistoscopic presentation, the memory systems are not individuated and thus learning of any sort has as strong an inhibitory effect as has learning of similar material. Differentiation has begun but is not complete with the time intervals of a few seconds used in the other series; dissimilar materials or even dissimilar activities affect each other, although by this time the strongest inhibition comes from material of the same type as that which is being inhibited.

It is likely that the speed of individuation is not constant, but depends upon the difficulty of the material.[6] An easy task is probably dissociated sooner than a more difficult one from the mass of unrelated activity in the organism; after a relatively short period it would therefore be resistant to inhibition. In a more difficult task the changes would be slower, as well as weaker. According to this reasoning, with tachistoscopic exposures all

[5] Representative experiments are: J. D. Frank and E. J. Ludvigh, The retroactive effect of pleasant and unpleasant odors on learning, this JOURNAL, 43, 1931, 102-108; L. M. Harden, The effect of emotional reactions upon retention, *J. Gen. Psychol.*, 3, 1930, 197-221 J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49.

[6] B. Ribsskog, Experimentelle Untersuchungen über die für den Unterrichtserfolg günstigste Repetitionszeit, *Zsch. f. angew. Psychol.*, 43, 1932, 301-347.

learning material is highly susceptible to inhibition, even from unlike material; with time intervals of a few seconds (as in Series I to XIII) easy material is less liable to inhibition from dissimilar presentations than is difficult material (lines were more readily inhibited by consonants then were consonants by the lines); and after longer periods this susceptibility lessens or disappears, as it does in most experiments. The theory thus accounts for the major findings of this experiment.

Conclusions

(1) When series of nonsense material are learned, as the series increase in difficulty there is (a) a retroactive inhibition of the earlier units which increases at first, but seems to reach a maximum, and (b) a proactive effect which may take the form of inhibition or of tilt.

(2) Proactive inhibition is weaker than retroactive inhibition.

(3) When experimental conditions are varied, parallel changes occur in the retroactive and the proactive effects.

(4) With time intervals of the order of 8 sec., dissimilar materials inhibit each other almost as much as similar ones.

(5) With time intervals of the order of 8 sec., multiplication causes both proactive and retroactive inhibition of learned material.

(6) With tachistoscopic exposure, inhibition is determined by the difficulty rather than the similarity of the material.

(7) Material that has been learned becomes individuated during retention.

(8) The speed of individuation becomes slower as the difficulty of the material increases.

EFFECT OF CROWDING OF MATERIAL ON CURVES OF FORGETTING

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In the effort merely to add greater precision to scientific findings, conditions are sometimes so altered that a new experimental situation is created, and to the problems originally envisaged are added new ones. In attempting to increase the statistical reliability of experiments on learning, for instance, often such large masses of items are used that interfering or disruptive forces among these items are introduced into the learning process. Facts established with single or isolated learning units are not always true for longer series of massed units. In the experiments to be reported in this paper, the effect of this massing of material on the conventional curves of forgetting has been investigated.[1] Two particular considerations motivated the study.

First, Jenkins and Dallenbach and Van Ormer have reported forgetting curves for intervals of sleep and waking.[2] In a comparison of the results obtained in these two investigations, it appears that the forgetting curves for intervals of waking have markedly different shapes. The curve in the study of Jenkins and Dallenbach shows a loss during the first hour of approximately 50% of the material originally learned, with a continued drop during the entire 8-hr. period until 90% has been lost. Van Ormer's curves, on the other hand, show a slightly greater loss, 55%, during the first hour, and then only a comparatively small drop for the remainder of the 8-hr. period to 75%. While it is possible that the inconsistency in these results is due to arbitrary differences in the technique of the experiments, it seems more probable that the larger amount of material, approximately three times as much, learned by Van Ormer's *Ss* at any one experimental session would have a bearing on the outcome of the experiment.

This view is supported by results reported by Woodworth, using lists of from 5 to 30 sets of paired words.[3] After 3 repetitions the list of 5 pairs

* Accepted for publication February 28, 1939.

[1] The experiments reported were conducted by members of undergraduate honors seminars at Swarthmore College. Credit is due to Margaret Chase, Peggy Harding, Ann Hargreaves, Helen Crosby, and Dagny Hoff for their respective shares in the experiments.

[2] J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49.

[3] R. S. Woodworth, The influence on retention of conditions favoring quickness of learning (abstract), *J. Philos.*, 12, 1915, 246. Cf. also, Woodworth, The better

showed 100% correct associations while the list of 30 pairs showed only 71% correct. Two days later the relative position of the lists was reversed; the list of 5 pairs had fallen to 4% correct, while the list of 30 pairs dropped only to 34%. Since the time devoted to each pair was equal in the two cases, Woodworth suggested increased effort on the part of the *Ss* to account for the clear-cut superiority of the longer list over the single list in the second recall. The present experiment was formulated, therefore, as an attempt to clarify this situation, that is, to test the effect of the amount of material learned on the shape of the forgetting curve.

Secondly, however, certain important theoretical considerations are involved. If forgetting is an expression of interference by events occurring in the daily experience of *S*, a masking of the original traces, then a considerable measure of such interference introduced intentionally on the occasion of the original learning should have a marked effect on the shape of the forgetting curve.[4] It might be assumed, for example, that material which has been mastered in spite of the strong intra-serial interference present in a long list would now be less subject to the relatively less important influences of subsequent chance events. Conversely, relatively isolated experiences would be under a continual detrimental influence because of the proportionately larger effect of the events of daily life. This hypothesis is doubtless too simple, as the facts reported below will suggest, but it indicates at least the general kind of relationship which must hold between these two conditions.

The Experiment

Two groups, totaling 21 students, learned by the anticipation method single lists of 8 nonsense syllables, and, at other times, 3 such lists of 8 syllables in quick succession. After intervals of 1, 24, and 48 hr. the *Ss* relearned the same lists. Retention was measured in terms of the conventional savings, that is to say, the difference between the trials necessary for the original learning to one perfect repetition, and trials for relearning to the same criterion. Each *S* served in 6 single experiments, one under each of the variations of the experimental conditions indicated above. The influence of the relative difficulty of the 12 different lists of material used and of the fairly sizable over-all practice efforts was equalized by rotation of both the lists and of the order of experiments for the various *Ss*. Thus 7 *Ss* relearned first after 1 hr., 7 other *Ss* first after 24 hr., and the remaining 7 after 48 hr.,—the order for single lists being the reverse of the order for the triple

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retention of longer memory lessons (abstract), *Psychol. Bull.*, 13, 1916, 65; and *Experimental Psychology*, 1939, 19-20, 56-60.

[4] The theory that forgetting depends upon interference has been clearly formulated by J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370. So far as the writer is aware, however, no critical review of the older facts about forgetting in terms of this theory has been published.

lists. Group I, consisting of 9 *Ss*, learned all 3 single lists before starting the triple lists. Group II, containing 12 *Ss*, learned the single and triple lists interspersed so as to balance practice effects in the original learning. The extent to which these controls were successful may be judged by comparing the rates of learning indicated in Table I below.

RESULTS

Original learning. The number of trials required by the two groups to learn the lists is given in Table I. In each case the results for all the lists learned before the 1-hr. interval are grouped together; results for the other

TABLE I

TRIALS REQUIRED FOR ORIGINAL LEARNING

(Each value is the mean of values for all 8-syllable lists learned by *Ss* in each group preceding the interval indicated.)

Group	Hours to relearning	Single lists	Triple lists
I	1	7.33	4.89
	24	7.78	5.30
	48	6.22	5.44
	Mean	7.11	5.21
	$S.D._{mean}$	.49	.20
II	1	8.50	6.97
	24	6.92	6.75
	48	6.83	6.64
	Mean	7.42	6.79
	$S.D._{mean}$	.71	.40

two intervals are arranged in the same way. The values in the table each represent the mean for all *Ss* in that group. Comparisons of these values, as well as the size of the standard deviations reported, indicate rather large variability from one *S* to the next. In addition, the rate of learning for the triple lists is significantly faster than the rate for the single lists in Group I. The procedure for this group indicates that this difference is due to an over-all practice effect. For the purposes of this paper, however, this difference is not of major importance, since we are interested in the slope of the forgetting curves; in the subsequent discussion, Group I and Group II are treated together. While the rates of learning for the various sub-groups are not as uniform as might be desirable for greatest reliability, the absence of systematic differences enables us to treat them as functionally equivalent.

Forgetting. Forgetting may be calculated either by finding the percentage saving for each individual *S* and then taking the mean of these percentages, or by averaging the actual number of trials for learning and for relearning and calculating the saving from these two means. In this experiment, the two methods give very nearly identical results, but since the

latter gives more concrete information it has been used in the subsequent presentation. In Table II are given the data relative to forgetting, and the final results are presented graphically in Fig. 1.

Four values are given for each of the six experimental conditions, the mean number of trials required for the original learning, the mean number

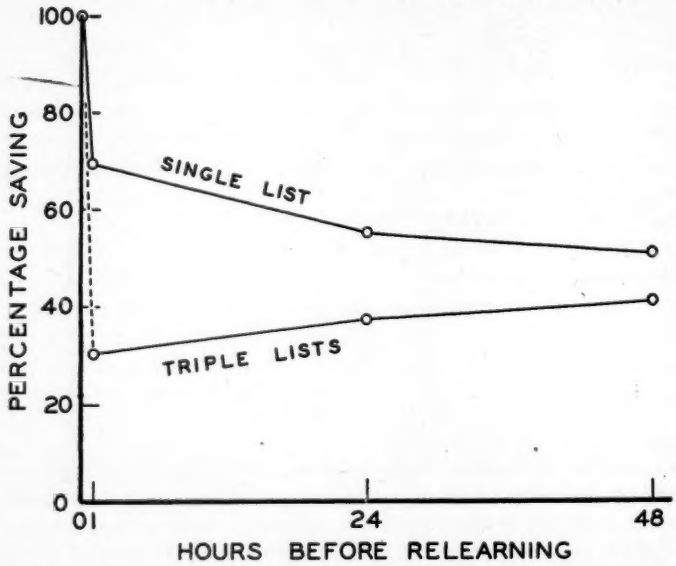

FIG. 1. CURVES OF FORGETTING, SHOWING RETENTION MEASURED BY SAVINGS UPON RELEARNING

The upper curve is for a single list of 8 syllables; the lower curve is for 3 lists of 8 syllables learned in quick succession.

of trials for relearning, the saving in terms of trials, and finally, the saving in percentage of trials for original learning. In the column headed "Difference" are given comparisons of the savings for the single list and the triple lists at each interval. The standard deviation and critical ratio for this difference has been calculated directly from the differences of the individual *Ss*. In the lower part of the table are presented, correspondingly, comparisons of savings for the 1-hr., 24-hr. and 48-hr. intervals, together with their critical ratios computed again directly from the differences for each individual subject.

At first glance it may be seen that the expectation of a marked differ-

ence in the slope of the forgetting curve for the single list and the triple lists is borne out. The triple lists show more than twice as much forgetting as the single list at the one-hour interval, only one and a half times as much as the single list at the 24-hr. interval, and but slightly more than the single list at the 48-hr. interval. The 1-hr. difference is highly reliable, the 24-hr. probably reliable, and the 48-hr. not reliable. Time obviously

TABLE II

TRIALS REQUIRED FOR LEARNING, RELEARNING, AND SAVINGS FOR INTERVALS OF 1, 24, AND 48 HR., FOR SINGLE LISTS AND FOR 3 LISTS OF 8 SYLLABLES LEARNED AT ONE TIME

(Critical ratios are calculated from $S.D._{diff.}$ found directly from differences for each S.)

Time		Single list	Triple list	Difference S–T	Critical ratio
1 hr.	Learning, trials	8.00	6.07		
	Relearning, trials	2.43	4.24		
	Savings, trials	5.57	1.83	3.74	4.37
	Savings, %	69.6	30.1	39.5	
24 hr.	Learning, trials	7.28	6.13		
	Relearning, trials	3.28	3.80		
	Savings, trials	4.00	2.33	1.67	2.46
	Savings, %	54.9	37.0	17.9	
48 hr.	Learning, trials	6.57	6.13		
	Relearning, trials	3.24	3.63		
	Savings, trials	3.33	2.50	.83	1.69
	Savings, %	50.7	40.7	10.0	
Difference (24–1), trials		−1.57	.50		
Critical ratio		2.16	1.35		
Difference (48–24), trials		− .67	.17		
Critical ratio		1.00	.41		
Difference (48–1), trials		−2.24	.67		
Critical ratio		4.77	2.31		

affects the two cases differently, working to the advantage of the triple lists and to the comparative disadvantage of the single list. This demonstrates, so far as is possible, that the difference between the forgetting curves of Jenkins and Dallenbach and of Van Ormer is a function of the amount of material each gave his *Ss* to learn, and it further gives us a significant clue to the relatively less clear-cut outcome of Van Ormer's experiment.

Much more striking was a result which appeared quite unanticipated by me. Some of the first results with the triple lists indicated a greater degree of saving at the 48-hr. interval than at the 1-hr. interval. This result was at first attributed to chance, but further experimentation only served to reenforce the original observation. A glance at Fig. 1 will show quite clearly that there is a continuous rise from the 1-hr. interval through the 24-hr. to the 48-hr. interval: more is remembered of the triple list the

longer the interval before it is tested. The differences are not large on an absolute scale, but the rise is so consistent from *S* to *S* that the approximately 10% increase between the 1-hr. and the 48-hr. intervals appears statistically significant, and it is probable that the smaller differences would prove reliable were a sufficient number of *S*s employed.

Even though this experiment was not set up so as to be formally comparable to the conventional experiments on reminiscence, the parallel between the results noted above and those designated by the term *reminiscence* is so striking as to suggest that they actually represent the same phenomenon.[5] Moreover, the procedure of this experiment did not actually require that at any one moment the *S*s should have complete mastery over all three of the triple lists. Thus the 100% point plotted for a zero time-interval and the steep slope of the curve from this point down to that for 1 hr. have no real significance, and we have to consider merely the trend of the curve between 1 and 48 hr., which shows clearly the parallel to which I have referred.

Discussion

It is possible to formulate certain logical consequences of these experimental findings.

In the first place, two general alternatives are available to explain the increase in performance on the triple lists with the passage of time: either the traces involved (or any equivalent facilitating agent) are strengthened, or an interference initially present is lessened. A strengthening might result from either a process of perseveration or consolidation, or from processes of growth or maturation. The increase in the retention of specific items which is present in the experiment could with difficulty be accounted for by either of these alternatives.[6] The second alternative, the lessening of an initial interfering or inhibiting process, seems at the present time far more likely. The initial interference might have something to do with factors like fatigue or loss of interest; or the inhibitory effect might have its

[5] The work on reminiscence has been adequately reviewed by G. O. McGeoch and will not be gone into here; cf. G. O. McGeoch, The conditions of reminiscence, this Journal, 47, 1935, 65-89.

[6] It cannot be denied that the improvement in certain types of performance may be adequately explained by a concept of maturation. James's example of learning to swim in the winter is too well known to need elucidation. Cf. R. H. Wheeler, *The Science of Psychology*, 1929, 290-300, 319-322, 495-496. Snoddy's experiments on mirror drawing have been cited in support of this theory: G. S. Snoddy, An experimental analysis of a case of trial and error learning in the human subject, *Psychol. Monog.*, 28, 1920, (no. 124), 1-78. On the other hand, the disappearance of Snoddy's "irradiation pattern" during a rest period may be similar to the subsidence of initial interference posited in this paper.

origin within the material itself, in the form of interaction among its items.[7] The last of these explanations is the only one which appears satisfactory, and I shall tentatively adopt it.

Second, since the single lists do not show "reminiscence," while the triple lists do, crowding of material is indicated as the condition which is specifically responsible for the interference postulated above.[8] Crowding introduces the probability of strong interference in the initial learning and therefore the possibility that such interference will subside in the course of time. With less crowding, this smaller amount of initial interference allows for but little change in time. One need not assume, of course, that such interference is entirely absent in the single lists. On the contrary, it is probably present in no inconsiderable amount. One need only postulate that the intra-serial interference in the single list is not so large that the facilitating effect of its removal will offset the decrement due to the chance inhibitory forces of ordinary forgetting. According to this view, any actual forgetting curve will be the resultant of (1) interfering or inhibitory processes accumulating in time, and (2) the decreasing effectiveness of any interference with the passage of time. The latter would include both interference present at the time of original learning, and that accruing in the process of forgetting.[9]

Third, the fate of the intra-serial interference may be specified to some degree. The results of the present experiment show that this interference gradually decreases during the period from 1 to 48 hr. after learning. It is doubtful whether the three points on the curve given above furnish any clue to the complete course of this function, but it is important to emphasize the relatively long period, certainly considerably more than one hour, during which "reminiscence" is found.

The conclusions which have been reached in this discussion are not entirely novel. In several important respects they agree with predictions

[7] The probability that fatigue plays a rôle in these experiments may be excluded by noting that the rate of learning for the triple lists is slightly lower than that for the single list in Group II when, if anything, it might be expected to be higher. This difference, although statistically unreliable, suggests a warming-up during the learning period rather than any fatigue.

[8] The fact of crowding is as old as Ebbinghaus but it has recently been given new meaning. Cf. H. von Restorff, Ueber die Wirkung von Bereichsbildung im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342; and C. L. Hull, The conflicting psychologies of learning—a way out, *Psychol. Rev.*, 42, 1935, 491-516. The experiments of Von Restorff are summarized in K. Koffka, *The Principles of Gestalt Psychology*, 1935, 481-493, and W. Köhler, *The Place of Value in a World of Fact*, 1938, 256-265.

[9] Retroactive and proactive inhibition should be added to chance interference as factors producing forgetting. A common basis in crowding, retroactive inhibition and forgetting is indicated. Cf. also Von Restorff, *op. cit.* 328-342.

made by Ward and by Hovland on the basis of deductions from so-called conditioning experiments.[10] Ward and Hovland have pointed to the temporary character of inhibition in the learning process as the source of the reminiscence effect. This intra-serial inhibition depends upon "inhibition of delay" derived from remote excitatory tendencies spanning any given syllable. Implicit in their assumptions is the conclusion that larger amounts of material should introduce proportionately greater amounts of inhibition. This last factor, however, has been relatively neglected in favor of the variable used by Hovland; namely, the spacing of the learning trials.[11] Unfortunately, the effect of spacing may be interpreted in two different ways. Either spacing in time may allow some kind of an interference or inhibition to disappear during the learning process so that a subsequent pause shows no additional effect, or the spacing may effectively eliminate the initial crowding or massing of material which produces the interference or inhibition. It seems important, therefore, to lay particular stress upon the fact that interference is a direct result of a crowding, or massing of homogeneous or near-homogeneous items in a particular learning process.

In one relevant respect, the present results disagree with those of Ward and Hovland. These writers are inclined to point to a very short period immediately following any learning process as being the period when one might expect to find most reminiscence. Perhaps this is because the parallel with conditioning is so alluring.[12] Since the first point on our curve represents a reproduction fully one hour after the end of the learning, and since a substantial and statistically reliable rise was found between this point and that for the 48-hr. interval, it is no longer possible to argue that the locus of reminiscence is in the immediate temporal vicinity of the learning process. To this extent, the present finding is an important corroberation of the shape of curves such as those published by Ballard.[13]

An effect closely akin to reminiscence has been described recently by

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[10] L. B. Ward, Reminiscence and rote learning, *Psychol. Monog.*, 49, 1937, (no. 4), 1-64; C. I. Hovland, Experimental studies in rote learning theory: I. Reminiscence following learning by massed and by distributed practice, *J. Exper. Psychol.*, 22, 1938, 201-224; II. Reminiscence with varying speeds of syllable presentation, *ibid.*, 338-353. Both experiments were based on predictions of Hull, *loc. cit.*

[11] This difficulty is also present in Hull's discussion where the effect of length of list on the amount of inhibition is nowhere explicitly recognized.

[12] Cf. W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 279-288. This view has led Hull to lean heavily upon a decrease in "excitatory tendency" to explain forgetting, a view at variance with the interference theory advanced by McGeoch and adopted in this paper.

[13] P. B. Ballard, Oblivescence and reminiscence, *Brit. J. Psychol. Monog. Suppl.*, 1, 1913, (no. 2), 1-82.

I. Müller in her investigation of "Reproduktionstörung."[14] In experiments arranged like those on retroactive inhibition, her results indicate a greater disturbance of recall when interpolated material is brought closer to the test. Since this disturbance is temporally associated with the recall, Müller has termed it "disturbance of reproduction" rather than retroactive inhibition, from which it would seem to differ. The parallel with reminiscence is shown in certain of her further experiments, in which the percentage of correct recall was quite consistently higher if the test followed the interpolated material not immediately, but after 12 min. The "disturbance of reproduction" behaves so much like reminiscence that, as Müller points out, parsimony would seem to demand a single explanation.

Evidence from these two earlier experiments, as well as from the present study, justifies the conclusion that the intensity of interference or inhibition produced by massing of material decreases to some extent with the passage of time. It is an effect which may take place within 2 min. after learning (Hovland and Ward), over a period of 12 min. (Müller) and over a period considerably greater than one hour. There is as yet no indication of the reasons why an initial inhibition should tend gradually to subside.

Summary

(1) The forgetting curve for a single list of 8 syllables was found to fall off to 65% retention in the first hour and to continue dropping to about 50% retention at the end of 48 hr. The forgetting curve found for 3 such lists learned in quick succession, on the other hand, showed a rapid drop to 30% during the first hour, followed by a continuous *rise* to 40% at the end of 48 hr.

(2) Earlier experiments dealing with forgetting have not taken adequate account of the influence on their results of the amount of material given *S* to learn.

(3) The rise in the curve for the triple lists, it is argued, is a result of the gradual subsidence of interfering forces which are produced by the massing or crowding of items in the original learning.

(4) 'Reminiscence' will appear, according to this view, when the magnitude of this recovery is greater than the loss due to the chance interference of events in daily life which otherwise produce forgetting.

(5) Interference may continue to subside for a period of well over an hour.

[14] I. Müller, Zur Analyse der Retentionsstörung durch Häufung, *Psychol. Forsch.*, 22, 1937, 180-210. Müller points out that her results indicate an interpretation of reminiscence in terms of interference (*op. cit.*, 208-210).

'SUBJECTIVE' COLORS FROM LINE-PATTERNS VIEWED IN POLYCHROMATIC AND MONOCHROMATIC LIGHT

By THEODERA M. ABEL, New York City

Color-effects are obtained from the stimulation of adjacent retinal fields by temporal patterns of white light as in the Benham disk and its modifications. These rotating stimulus-patterns carry alternation of black and white in various proportions.[1] The explanation of the color phenomenon is that the chromatic constituents of 'white' grow and decay at different rates.[2] The hue, saturation, and chroma of the color observed on a particular concentric ring of the disk vary also with the duration and intensity of stimulation and speed and direction of rotation.[3]

This same color phenomenon can also be obtained, although not as vividly, from the observation of stationary stimulus-patterns, as a black and white tile floor or the Luckiesh-Moss line pattern consisting of parallel diagonal black and white lines.[4] In just what way hue is a function of time in a stationary pattern is not entirely clear. Dallenbach and Erb have made a thorough study of the Luckiesh-Moss pattern and have found that the 'subjective' colors observed in the pattern are to some extent dependent upon eye movement, but eye movement, though necessary, does not appear to be a sufficient or compulsory condition.[5] At least we can venture to say that since eye movements seem necessary to the production of the color-effects, line patterns are not really stationary but in some way, not entirely clear, allow for intermittent stimulation of light on adjacent retinal fields as in rotating stimulus-patterns.

The illumination under which observations of this color phenomenon

* Accepted for publication September 1, 1935.

[1] Diagrams of the various disks used in the production of "subjective colors" are given by Erb and Dallenbach, 'Subjective' colors from line-patterns, this JOURNAL, 52, 1939, 229-230.

[2] L. T. Troland, Vision: 1. Visual phenomena and their stimulus correlations, *The Foundations of Experimental Psychology*, 1929, 200.

[3] The most thorough study of the control of variables in producing color-effects from rotating disks of light and dark patterns was made by G. A. Fry. Cf. Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492; Color phenomena from adjacent retinal areas for different temporal patterns of intermittent white light, *ibid.*, 714-721; Color sensations produced by intermittent white light and the three-component theory of color-vision, *ibid.*, 46, 1934, 464-469.

[4] M. Luckiesh and F. K. Moss, A demonstrational test of vision, this JOURNAL, 45, 1933, 137 ff.

[5] M. B. Erb and K. M. Dallenbach, 'Subjective' colors from line-patterns, this JOURNAL, 52, 1939, 240-241.

have been made has usually been white light or day light. A few experimenters, however, have reported color-effects from rotating black and white stimulus-patterns in polychromatic and monochromatic light. Here, of course, it is not a question of breaking down the constituents of 'white' light, since there is no 'white' light present.

The first reference to the employment of illumination other than white light was made by Benham.[6] He had objected to Liveing's physiological interpretation of the phenomenon of perceiving different colors on the Benham disk. Liveing stated that the eye perceives colors more quickly at one end of the spectrum than at the other. Benham claimed that the color-effects were "artificial sensations" as he had observed them when his disk or top was illuminated by a bright sodium flame and that they could not be explained by physiological functioning of the eye alone. Liveing repeated the experiment of Benham and reported seeing only dark brown, pale brown and gray, no red, blue and green as in white light.[7] But he also employed as stimulus, spiral black figures on a white background in sodium light. Some observers reported a play of colors; he did not. Liveing then conceded that the phenomenon was 'subjective' and varied from individual to individual.

Abney observed the Benham disk in gas light and through colored filters giving relative monochromatic light.[8] He also made white light by mixing complements. In all cases he found some color-effects on the different concentric rings of the disk, but in monochromatic illumination the variation of colors was very slight and with some lights, such as green and violet, he saw only the concentric rings as different tints of the color of the illumination.

Turner painted the Benham pattern on a glass disk mounted on a revolving stage and rotated it in a projection lantern by means of a multiplying wheel.[9] He reported a great variety of effects if colored glass was interposed in the path of the beam. For example, with blue glass in gas light and a moderate rate of rotation, the concentric rings were red, gray, green, and blue. With monochromatic red glass the lines were blue, gray, red, and dark red. He was puzzled to find a rich blue under monochromatic red illumination, but he did not attempt to explain the phenomenon.

The most thorough work done with monochromatic light was that of Piéron.[10] He employed Wratten filters through which he observed several modifications of the Benham disk. Unequal opacity of the filters prevented a comparison of the colors in equivalent conditions from the point of view of aberration (lag) of the different colors, but by varying the speed of rotation he obtained an optimal visibility for each filter. With R and O filters Piéron obtained very good colors. With B and G filters, the colors were poor. The colors varied with the speed of rotation as in white light. Piéron questions whether the mechanism is the same that produces the color-effects in white light. He suggests that most likely all the spectral regions are

[6] Benham, The artificial spectrum top, *Nature*, 51, 1894, 200.
[7] Liveing, *ibid.*, 200.
[8] W. de W. Abney, The artificial spectrum top, *ibid.*, 292.
[9] Dawson Turner, The spectrum top, *ibid.*, 439.
[10] Henri Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham, *Année psychol.*, 23, 1922, 1-49.

susceptible of acting on all fundamental chromatic processes, but with unequal intensity (power).[11]

The phenomenon of flight of colors after exposure to white light would seem to be related in some way to the phenomenon of 'subjective' colors, although the typical flight of colors including those other than the positive and negative after images appears only after longer and more intense stimulation than the color-effects from the more immediate and weaker stimulation of black and white patterns. Flight of colors has also been observed as a result of monochromatic (sodium-yellow in the arc spectrum) stimulation of the retina.[12] No explanation, however, was offered for this phenomenon.

No work has been done in observing a stationary black and white pattern in monochromatic or polychromatic illumination other than daylight. Dallenbach and Erb made a thorough study of the subjective colors from the Luckiesh-Moss line pattern under various conditions, but in artificial daylight illumination only.[13] They found, in the first place, that the pattern of narrow black lines close together on a white background gives rise, when regarded steadily for short periods of time, to the perception of pastel colors, usually of low saturation and light tint. The lines were predominantly yellow and blue, but covered the entire spectral range. These colors appeared to both naïve and sophisticated *Os*. The colors were more pronounced when fixation was not in the plane of the figure. They modified the width of the diagonal black lines and the intervening white spaces, and they varied the color of the lines and background (*i.e.* blue lines on white, etc.), but no condition was as effective as the original black-white pattern. The *Os* all reported some kind of phenomenal movement across or on or between the black lines.

Our problem was to supplement the work of Dallenbach and Erb by determining the effect of observing the original line pattern of Luckiesh and Moss in colored light, both polychromatic and monochromatic.

Procedure. Our general procedure was the same as in the experiment of Dallenbach and Erb.[14] The stimulus-figure (see Fig. 1) was a pattern of diagonal lines about 0.5 mm. wide and about 1 mm. apart, covering a rectangle 9 x 6.5 cm. and printed in dead black on egg-shell white paper. This pattern was presented to the *Os* for 1 min. by means of a simple exposure-apparatus. The following colored lights were employed:

(1) Polychromatic:
   (a) Tungsten lamp in which all colors are emitted in varying proportions, the preponderance being at the red end of the spectrum.

[11] *Ibid.*, 36.
[12] A. Ford, Duration of stimulation and the flight of after-images from sodium-yellow in the arc spectrum. *Psychol. Bull.*, 28, 1931, 709.
[13] *Op. cit.*, 227-241.
[14] *Ibid.*, 234.

(b) Quartz mercury arc lamp that contains very little red, fairly bright Y-O line (577 and 579 mμ), bright Y-G (546 mμ), faint B-G (492 mμ), strong B (436 mμ) and violet low in visibility (405 mμ).

(2) Monochromatic:

(a) Yellow light. Filter: amber OG-3 used with illumination by quartz mercury arc lamp. Radiations almost exclusively 577 and 579 mμ and a very little red at 623 and 691 mμ.

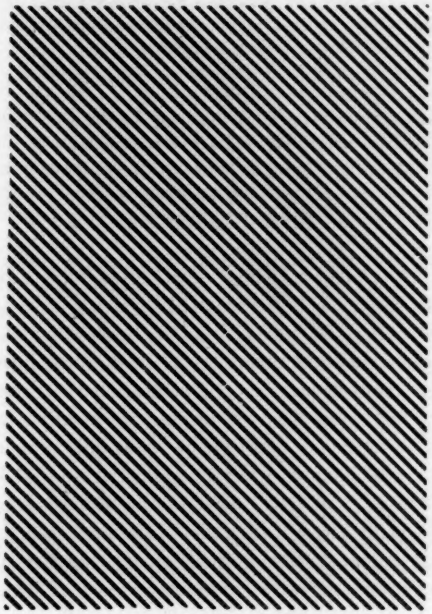

FIG. 1. THE STIMULUS-PATTERN

(b) Green light. Filter: liquid in a circular parallel sided quartz cell with illumination by quartz mercury arc lamp. Radiation at 546 mμ with a minute amount of 577 and 579 mμ.

(c) Red light. Filter: H-R traffic red with illumination by Tungsten light. Transmits light at 623 and 691 mμ.

The filters were placed in a pasteboard eyepiece so that the stimulus-card was observed monocularly with the right eye. The intensity of light reaching the eye was kept constant for all the light sources. The necessary intensity of light falling on the stimulus-card, in order to allow for this constant intensity of light reaching the eye (with or without the filters) was determined in advance.[15] Adjustments were then

[15] The foot-candle power falling on stimulus-card necessary to produce an equal intensity of light penetrating the eye: mercury arc plain, 4.1; amber filter (yellow),

made by varying the distance of the course of light that was placed behind, but slightly above *O*'s head, and by reading the intensity in terms of foot-candle power from a sight meter placed on the stimulus-card. *O*'s eyes were always at a distance of approximately 14 in. from the center of the stimulus-pattern when exposed.

Five *O*s served under these five experimental conditions. Two of these *O*s had observed in Dallenbach and Erb's experiment. Besides these 5 *O*'s, two others served in certain parts of the experiment—one with the Tungsten lamp and the red filter, the other with the mercury arc lamp and the yellow and green filters. Six of our *O*s were trained psychologists and one was a physicist.

Every *O* was first told to look at the stimulus-pattern which was to be exposed directly in front of him and describe what he observed. Secondly, he was told to look at the stimulus-pattern with far-fixation and do the same thing. Each *O* observed the stimulus-card once under the different conditions of illumination. The order of presentation of the different light conditions was: mercury arc plain; yellow filter; green filter; tungsten plain; red filter. The experiment was carried on during the summer of 1934 in the physical and psychological laboratories of Cornell University.

Results. As in the case of daylight illumination, both 'subjective' colors and phenomenal movement were reported by all *O*s at some time during the exposure situations. The main results can be classified in the following manner: (1) Differentiation among the black diagonal lines both of hue and of brightness (astygmatic effect). Two *O*s stated that this phenomenon occurred particularly when the eyes assumed the position for far-fixation. (2) Color in the interstices between the black lines, either filling in all the space between the lines or merely along the edges of the lines. (3) Patterned lines (figures or designs, sometimes beaded, sometimes streaming, in motion) diagonally across the parallel black lines. Sometimes these patterns were black or gray, but more often colored, pastel shades. (4) When color phenomena were reported, continual change of the color was observed, generally in the direction of an increase in saturation.

Not all the *O*s reported the phenomena above, nor did any one *O* always report all these phenomena at a given time, although this did occur.

Under all conditions, except monochromatic red, many color phenomena were reported. With the polychromatic illumination, the colors were generally more vivid and saturated than in the monochromatic light. Even in the yellow and green monochromatic lights, however, color phenomena were observed that were not limited to the color of the light source nor to its negative after-image. For instance, in the yellow light, red, green, dull orange, reddish yellow, yellowish green and blue (purple is the nega-

12.2; liquid filter (green), 47; tungsten plain, 3.5; red traffic filter, 27.3. My thanks are due Dr. Roger Estey for providing the filters and making the photometric determinations.

tive after-image of this yellow) were observed at different times. With the green light, yellow, bluish green, red, brown and blue (violet is the negative after-image of this green) were observed.

With the red light, however, only various shades of red (one instance of orange) were observed. The streaming phenomenon across the diagonal black lines was reported, but this was either a gray pattern or reddish.

Unfortunately, all the observations in this experiment had to be carried on under conditions of low illumination because of the low intensity of light capable of penetrating the filters, particularly the red filter. Consequently, the intensity of illumination was in no way comparable to that in the experiment with the daylight illumination. It will be a matter for further investigation to find out the influence of the intensity of the illumination on the nature of the color phenomena observed.[16]

Summary. We may summarize the results of this study as follows:

(1) The same phenomena were observed in colored light sources as in the previous experiment when daylight illumination was employed.

(2) The color phenomena reported in polychromatic illumination are apparently analogous to the phenomena reported by other experimenters who have employed temporal patterns of intermittent stimulation of 'white' light upon adjacent retinal fields.[17]

(3) The color-effects reported in the present experiment under monochromatic illumination need further explanation than that of aberration of polychromatic light in a temporal sequence, since no polychromatic light is or was present. The tri-receptor theory of Young-Helmholtz states that a given receptor in the retina is not only sensitive to a given wave-length but is also sensitive to the other wave-lengths of the spectrum, but to a less degree.[18] Consequently, it is conceivable that under certain conditions not well understood of intermittent stimulation by temporal and spatial patterns, in monochromatic light, the equilibrium of the receptors (cones) is broken down in some way beyond that of the complementary balance so that they respond as though stimulated by different wave-lengths.

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[16] With illumination of greater intensity various color phenomena were reported with the red filter.

[17] The 'white,' of course, is not always observed as white but as colored by the predominant hue in the polychromatic illumination, *i.e.* red with illumination by the Tungsten lamp.

[18] S. Hecht, The retinal processes concerned with visual acuity and color vision, *Bull. No. 4, Howe Laboratory of Ophthalmology, Harvard Medical School,* 1931, 67-80.

NEW METHODS OF OBTAINING NEUROTIC BEHAVIOR IN RATS

By GEORGE HUMPHREY and FRED MARCUSE, Queen's University

In a recent monograph,[1] Maier described certain disordered activities that he obtained in the rat. We have succeeded in duplicating his results by techniques entirely different from his. Maier used Lashley's jumping-board technique, according to which the rat jumps from a platform at one of two patterns, behind one of which is food. Acutely disordered behavior ("neurotic") was produced when one pattern was removed, leaving the animal no choice but at the same time forcing it to jump by turning a jet of air upon it.

We have used a new series of stimuli, graded in severity, partly on normal rats, partly on a small group of animals in which chronically disordered behavior has been induced by a method to be described. The method apparently permits of differentiation between these two groups, and clearly contrasts what may be called a chronic and a traumatic stage of abnormal behavior.

In order to induce chronically disordered behavior, 10 rats were trained by daily runs for 25 days in a Warner-Warden multiple Y-maze, set up in the type left, right, left, right, foodbox. The foodbox had no bottom so that the rat and its food were in direct contact with the floor of the room. With 6 of the animals the foodbox was moved along the floor, after the animal was in the box and the door closed. Movement was carefully effected so as not to cause pain; the extent of movement varied from 4 to 10 ft., with no appreciable effect on immediate behavior, except that the animals did not eat until movement ceased. The relation of this movement to the animal was something like that of a revolving door, which is being pushed by someone else, to a pedestrian. The remaining 4 rats were trained in the ordinary way, with a stationary foodbox.

Results. (a) Chronically disordered behavior. The 6 rats whose foodboxes were moved were consistently above the 4 unmoved controls in their scores, both for time and error. The 4 controls satisfied the criterion for learning; namely, 5 errorless runs, within 16 days, but not one of the 6 experimental rats did so within 25 days. The curves for the experimental animals are strikingly more irregular than those of the controls, both in time and errors. At the same time there appeared in the experi-

* Accepted for publication August 10, 1939.

[1] N. R. F. Maier, *Studies of Abnormal Behavior in the Rat*, 1939, 1-81.

mental animals the following activities which are significant of disorder. (1) "Belly-crawling"—*i.e.* crawling upon the belly through part of the maze or when finally moving into the foodbox—was observed in the behavior of all 6 experimental animals and on several occasions by each of them. (2) Loud gnashing of the teeth was frequently heard—louder than the chattering that these animals ordinarily make. (3) Spasmodic starts from one side to the other of a cul-de-sac were common; as was also (4) shivering. There was (5) frequent biting of the door of the foodbox and also at the metal mesh at the top of the culs-de-sac. The rats (6) frequently moved hesitatingly back and forth a few inches at a time when they arrived at the entrance to the foodbox. When half way into the foodbox, they would frequently (7) withdraw—sometimes completely retracing the maze (with or without errors) and then return again to the foodbox (with or without errors). All these actions were observed in all the rats during the 25 days of their training.[2] None of them was observed in any of the control animals.

Further analysis showed that while the experimental animals never learned the maze in the sense that they achieved errorless runs into the foodbox, 5 of the 6 learned to run *as far as the door* of the foodbox without error. In that they achieved the criterion of 5 errorless runs by times varying from the 12th to the 27th day. That is to say, these animals ran to the door of the foodbox, probably to within sight of the food, without a mistake—and did that on 5 successive trials. They then retraced their steps making mistakes, behaving abnormally and so on, spending in this way the rest of the time before they entered the foodbox. The sixth experimental animal did not even learn to reach the door of the foodbox without error during the 25 trials.

It is hard not to use anthropomorphic terms in describing these animals. The most natural description seems to be that the animals *knew the way to the foodbox but were prevented from entering it by some conflicting motive.* In terms lately made fashionable by Lewin, one may say that there was apparently present a cognitive structure corresponding to the maze, with conflicting valencies at the foodbox. The conflict is evidently one which is very difficult, perhaps impossible for the animal to resolve.[3] It

[2] No attempt was made to make a quantitative record of such behavior. The records show teeth chattering, 22 observations; belly crawling, 20; spasmodic starts, 16; shivering, 15; biting at door, 14. Since more than one person was engaged in training the rats, which in the case of the abnormal animals is a very protracted business, and since no scheme of annotation was adopted until late in the experiment, these figures are suggestive rather than exact.

[3] Except by some special device, such as was adopted by one rat who made a practice of snatching out the food before the door of the foodbox could be closed.

apparently induces a condition of 'nervous strain' which results in the chronically disordered activity above described. It may be mentioned that 85 rats were handled during the experiment by the junior author, who was bitten 4 times in all, each time by one of the chronically disordered animals. Total daily errors of a separate group of 4 animals trained similarly were temporarily reduced, by 2 cc. of a 10% solution of alcohol, from 9 to 0, and the average running-time was reduced from 22 sec. to 7 sec. These effects occurred the day after the alcohol was given them—the 38th day of the experiment. A similar result was obtained after a second dose of alcohol—given 5 days later. This finding is reported as suggestive rather than as scientifically conclusive.

(*b*) *Acutely disordered behavior.* Maier reports highly excitable behavior in his rats, followed by a plastic phase, in which the animal could be taken in the hand lying on its back, and its limbs 'moulded' by the experimenter. We have obtained these effects by a procedure different from his. Stimuli of three grades were used. S_1 consisted of ringing an electric bell (2 dry cells) for 3 min. on the *floor* of the carton on the upper surface of which the animal was placed. S_2 consisted of ringing the bell for 3 min. close up to the cage, and on the same plane with it. S_3 consisted in swinging the cage gently 5 or 6 times with an oscillation of 180° while the bell was ringing in the position of S_2. Thus S_1, S_2, and S_3 were of progressive degrees of 'severity.' Experiments were mainly confined to S_1 and S_3, though seizures were obtained by all three methods. Table I shows the results obtained at the first trial of the stimulus in question. S_2 was not used at a first trial.

TABLE I

NUMBER OF SEIZURES OBTAINED AT FIRST TRIAL OF THE VARIOUS STIMULUS-GRADES

Rats	Stimulus-grade	No. males	No. females	No. seizures
Normal	S_1	24	—	0
	S_1	—	26	1
	S_3	23	—	11
Chron. dis.	S_1	6	—	3

It will be seen that while S_3 induces seizures in nearly 50% of normal male rats, S_1 has no effect on 24 males and induces a seizure in only 1 of 26 females. On the other hand, S_1 induces seizures in 3 (*i.e.* 50%) of 6 chronically disordered males.

This was a rational solution of the problem; the animal obtained the food without being moved. The records show that this animal's performance improved in time and errors after he had found this solution and before means were taken to bar it.

(c) Nature of seizure. The seizure as observed by us consists essentially in very violent motor activity. The animal begins by violent jumping round the cage, from one to another of the four walls, hardly pausing between each jump. Sometimes he appears to wound his nose during this behavior. When the cage door is opened, he leaps out on to the floor, rushes madly round and round in circles, or straight across the room, bumping headlong into chair legs and walls, and at times making great leaps across the floor. There is sometimes a peculiar noise, between a grunt and a squeak. Sometimes the rat leaps up directly into the air. One stunned itself for over a minute. Functional paralysis of the front legs often developed during seizure. In all, 40 seizures were observed. Of these, 28 were obtained from 3 rats; and of these 28, 6 were produced by S_1, 3 by S_2, and the rest (19) by S_3. All three of these disordered rats became adapted to S_1 and S_2 after two or three stimulations, and thereafter S_3 was used.

(d) Plastic phase. As observed by us, the plastic phase consists in complete inactivity immediately succeeding the excited phase. The animal can be taken in the hand, laid on its back and its legs stretched out or moulded,to use Maier's term. The feet and tail are bluish, the nose red, possibly owing to wounding in the cage. The hind toes are often rigidly outstretched, in contrast to the closed toes of the front feet. There is loss of the reflex to tickling the feet and in three cases there was loss of the blink reflex to a puff of air. A sharp pinch of the tail or pulling the vibrissae has no effect. The neck is drawn up in a peculiarly stiff posture.

Summary. Acute neurotic behavior was produced on 40 occasions in 15 rats, by a graded series of stimuli. The least 'severe' stimulus, produced seizures in 3 of 6 chronically disordered male rats, had no effect on 24 normal males, and produced a seizure in one of 26 normal females. The most 'severe' stimulus produced seizures in 11 of 23 normal males. The rats were throughout in good physical condition, as far as could be judged. Experiments are now proceeding to effect a standardization of the stimuli and, since the number of the chronically disordered animals is admittedly small, to confirm the results on a larger scale.

APPARATUS

THE NEUROVOLTMETER

By EDMUND JACOBSON, Chicago, Illinois

One of the greatest needs today in clinical as well as in research medicine is a convenient method and instrument for the measurement of nervous and of muscular states in man. With such an instrument the worker can enter fields new in the sense that they have seen much speculation but relatively little that can be measured in the clinic or in the laboratory. What happens in an individual when he is nervous or tense is obviously of great importance to the general practitioner as well as to the specialist. The complexities are so great, however, that little more than a beginning has been made. Workers interested in problems of nervousness may be tempted to begin with a definition; but the route here preferred is to secure measured data which will lead toward a definition.

In previous articles, I have shown that it is possible to measure what is clinically called 'nervousness' in intact man. This is accomplished without the use of an anesthetic, but also without noteworthy discomfort or psychic disturbance. Measurements of action-potentials are made in muscles or in nerves as desired. The *Ss* have included normal persons, but also patients suffering from various disorders, including neurotic or so-called 'nervous' or 'hypertensive' individuals. I have described methods whereby action-potentials from one or more muscles or nerves can be photographically recorded over a considerable period of time such as an hour.[1] The results can be presented in various ways; namely, as the photographs themselves, or as a curve in which action-potentials are plotted against time. A third manner of expressing the results is to average the peak microvoltage per unit of time (⅕ sec.) for the desired period of test and to express the result as a single number. For example, an individual who, during a given hour of test, showed an average of 5 to 10 microvolts certainly was very tense or nervous throughout much or all of the period. On the other hand, if the number was about one microvolt or under, the average was not far from that secured from a group of unselected or so called 'normal' individuals.

For these purposes, either a string galvanometer or an oscillograph with a suitable amplifier assembly can be employed. I have preferred the string galvanometer. It is necessary to have the apparatus sufficiently stable so that the recording string or wire vibrates very little when a short circuit exists across the input of the amplifier. Indeed, the vibrations of the string should not at the most

* From the Laboratory of Clinical Physiology. Read before the American Association for the Advancement of Science, Section I, Psychology, June 22, 1937.

[1] Edmund Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man; studies on arm flexors, *Amer. J. Physiol.*, 107, 1934, 230-248; Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man: relaxation-times of individuals, *ibid.*, 108, 1934, 573-580; Electrical measurement of activities in nerve and muscle, in *The Problem of Mental Disorder*, ed. by M. Bentley and E. V. Cowdry, 1934, 133-145; Measurement of the action-potentials in the peripheral nerves of man without anesthetic, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 713-715; The course of relaxation in muscles of athletes, this JOURNAL, 48, 1936, 98-108.

exceed 2 or 3 mm. in either direction. What is required, however, with this degree of stability is that the string should show a definite excursion from the zero axis of at least several millimeters per microvolt p.d. impressed upon the input terminals of the amplifier.

With such methods of recording it is a long and arduous task to secure a graph representing the variations of the action-potentials from a nerve or muscle during the period of an hour or so. Evidently it would be useful if we could secure some instrument which would simplify this task by integrating the voltages at a particular

FIG. 1. THE NEUROVOLTMETER

instant. In Fig. 1, is a photograph of the instrument designed to perform this sort of integration. This has been worked out with the collaboration of the Bell Telephone Laboratories including the help of Messrs. H. A. Frederick, D. G. Blattner, and H. M. Stoller, to whom I wish to acknowledge great indebtedness.

The method for the use of this instrument is extremely simple and convenient. Fine platinum-iridium wires with very sharp points are inserted into whatever muscle tissue or nerve is selected for measurement purposes. These wires are protected with insulating tape so that there is no exposure of platinum to air. Both the wires and the skin are previously sterilized with alcohol. Because the wires are very fine as well as sharpened, they are readily inserted with no marked discomfort and the patient does not as a rule complain about their presence. Surface electrodes

can be used, if preferred, thereby avoiding penetrating the skin. Leads connect the wires to the input of an amplifier. The amplifiers I employ most commonly show a fairly flat characteristic from about thirty to four thousand. The amplifier is connected to the input of the instrument which is to be seen in Fig. 1. A diagram of the circuit is shown in Fig. 2.

It is necessary to be able to vary the sensitivity of the meter and this can be done very readily by means of a variable shunt placed across the meter or across the entire circuit. If the patient is relatively calm and relaxed no shunt at all will be required; but if the patient is very excitable or tense the needle will at once shoot up to the highest point unless a shunt is used.

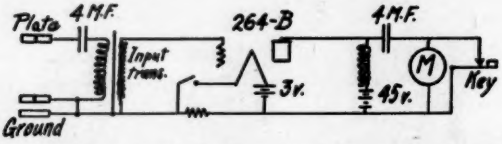


FIG. 2. CIRCUIT DIAGRAM OF THE NEUROVOLTMETER

The input terminals, plate and ground, are connected with the output terminals of the amplifier described previously, which delivers an open circuit voltage on the order of 7 millivolts. The input transformer has an approximately 15:1 step-up to the grid of a 246B vacuum tube. This tube operates on 0.3 ampere at 1.5 volts on the filament and 45 volts, 2 milliamperes in the plate circuit. The microammeter is connected in series with a 4 microfarad blocking condenser across the choke coil in the plate circuit. A strap key is provided which normally short-circuits this meter to protect it from transient disturbances. With 7 millivolts applied to the input terminals of the amplifier the meter reads approximately 20 microamperes. This makes it possible to cover a range of input voltages from 2 to 25 millivolts impressed upon the amplifier. By means of an adjustable shunt (1-10000 ohms) on the output of this amplifier, the necessary range for all measurements of nerve and muscle potentials can be covered.

The actual procedure in making a measurement is simply to insert the electrodes in a patient who may be lying down or sitting up as desired, press the key and read the meter. The preparations require but a few minutes and the meter affords instantaneous readings.

Meter readings secured with any particular shunt resistance can be standardized readily. An essential step is to read the meter when a short circuit exists across the input of the amplifier. For example, with a shunt of 3000 ohms, the meter reading is 2. When 1 microvolt is impressed the reading is 6, and when 10 microvolts are impressed the reading is 68. There is a little discrepancy here, showing that a little more work will need to be done to correct this point. We find, however, that the needle deflection is approximately 7 scale-divisions on the micro-ammeter per microvolt.

Seven scale-divisions is a large deflection for only one millionth of a volt. To effect this deflection, as you will have noted from the circuit diagram, the potentials from the electrodes in the nerve or muscle tissue are rectified. In other words we are securing a power reading and this should prove very useful for many research purposes as well as for divers needs in medical practice.

Fig. 3 illustrates how the instrument can be standardized for photography of the deflections of the needle. It shows how far, in millimeters, the needle moves for

one microvolt (Fig. 3a) and for 10 microvolts (Fig. 3b) and it shows how tightly the string is set (Fig. 3c). Photographs can be made if the meter is removed from the cabinet and placed in the path of the rays from the lamp of the string galvanometer. A small projecting rod, such as a broomstraw, is fastened perpendicularly to the tip of the needle in order to record the position of the needle on the same photograph as shows the string shadow. This changes the natural period somewhat, but we can ignore this change, since it is not designed for common use but only to permit us here to illustrate some of the characteristics of this meter.

For muscular studies in man, it is convenient to insert one electrode (positive) in the right biceps muscle near the midline (but below the "nervous equator" of Hermann) and the other under the skin in the olecranon fossa. This has been done in

FIG. 3. RESPONSE OF NEUROVOLTMETER

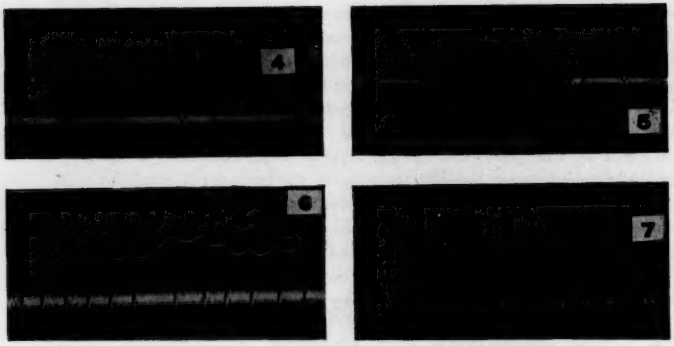
(a) To 1 microvolt, 57 cycles. Position of the needle indicated by arrow corresponding with meter reading 2 microamperes. (b) Arrow indicates position of needle responding to 10 microvolts. Meter reading 68 micro-amperes. (c) Speed of photographic paper, distance between vertical time lines = 0.2 sec. String tension, approximately 3 millivolts per centimeter. These features apply to Figs. 4, 5, 6, and 7.

preparation for Fig. 4, which illustrates complete relaxation in the forearm flexors as shown by the string galvanometer as well as by the position of this needle. Here, as may be observed, the needle follows a quiet course corresponding with that of the string.

As will be recalled from studies previously published from this laboratory, mental activity itself can now be measured, in so far as its occurrence characteristically includes activity in peripheral nerves or muscles. Fig. 5 illustrates what commonly happens when *S* merely imagines lifting a 10 lb. weight in his right hand. As will be seen, the deflections of the string become very marked while the excursion of the needle evidently integrates the same potentials. To be sure, the period of the needle is relatively slow: there is a considerable lag before the needle returns to zero, following the recurrence of complete relaxation.

A record from the same muscles in a patient who is somewhat tense is shown in Fig. 6. Several years after a skull fracture sustained by compression from a folding bed, she continued to be in a somewhat over-emotional or 'nervous' state, although her outward demeanor appeared calm. Here, although she is trying to be as quiet as possible, the string shows marked fluctuations continuously and irregularly and the meter reading is continually up in the neighborhood of 4 microvolts much of the time. Another illustration from the same patient is shown in Fig. 7. At a later date, when this patient had become less nervous, the readings are more like those shown in Fig. 4.

As stated previously, the new instrument should have many uses for the medical practitioner as well as for the research student. Therewith he can determine pre-



FIGS. 4-7. SAMPLE RECORDS

Fig. 4. Complete relaxation in a muscle as shown by the string galvanometer (lower tracing) as well as by the position of the needle (upper tracing). Voltage sensitivity of the string galvanometer assembly approximately 7 millimeters per microvolt, A.C. in this and in each of the figures following. Read all figures left to right. In all records appearing in this article, *S* is lying quietly, with eyelids closed.

Fig. 5. Action-potentials in muscles which flex the forearm when *S* merely imagines lifting a 10 lb. weight in his right hand. Signal line at the bottom indicates clicks of telegraph key; the first, at the beginning of the line, indicates to *S* that he is to imagine lifting a weight; the second, at the end of the line, that he is to relax any tensions present. Electrodes are inserted in the right biceps muscle region. The middle tracing, made by the string galvanometer, indicates action-potentials from the right forearm flexors (as in lifting), beginning about one second after the signal to imagine and ceasing soon after the signal to relax. The remaining tracing, which begins on the left near the top, is made with the neurovoltmeter, using a shunt resistance of 3000 ohms. As will be seen, it greatly resembles the form of a mechanical record of muscular contraction. (Over the beginning of the signal line a brief excursion appears in each tracing, which is to be ignored, since it is due to an inductive effect upon closure of the signal circuit.)

Fig. 6. Record from the right biceps-brachial muscles in a somewhat tense or 'nervous' patient. Note the action-potentials shown in the lower tracing (string galvanometer), including the recurrent firing in one group, overshooting the others in amplitude. Note also the parallelism in the middle tracing (neurovoltmeter). No shunt across the neurovoltmeter. Scale reading, 13 micro-amperes per microvolt, A.C. The short-circuit line (zero reading) lies about 6 mm. from the top of this record.

Fig. 7. Another sample from the same patient. Note the action-potentials, continuously varying around about 2-3 microvolts (lower tracing: from the string galvanometer), while a parallel recording of the same muscle tension appears above from the neurovoltmeter, which is here set at a somewhat less voltage sensitivity than in Fig. 6. The short-circuit line (zero reading) lies about 1.5 mm. above the upper portions of the neurovoltmeter tracing.

cisely how nervous or tense his patient is at a particular time when he wishes to make a diagnosis. He can test also the effects of any form of treatment which he has chosen to employ so far as they are shown in the nervous or muscular system. In these various ways the instrument may prove of service for treatment as well as for diagnosis.

SUPPLEMENTARY EQUIPMENT USEFUL IN RESEARCH ON EYE MOVEMENT

By JAMES S. KARSLAKE and JOSEPH TIFFIN, Purdue University

Three pieces of equipment, recently developed in connection with research in eye movement, that are also of interest to the general laboratory technician, are as follows: *(1)* exposure-device; *(2)* tachistoscope with remote control switch; and

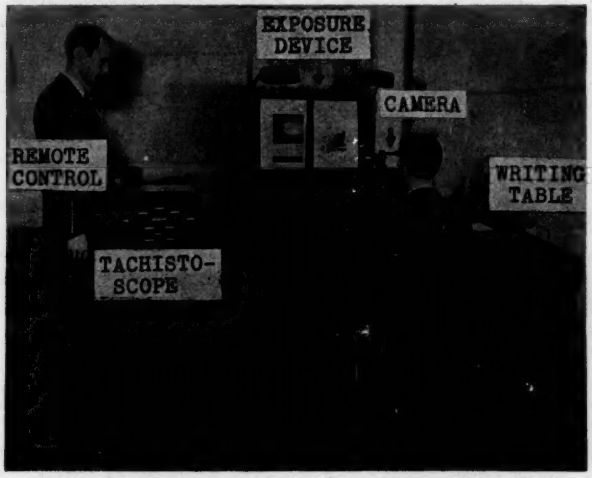


FIG. 1. SUPPLEMENTARY EQUIPMENT FOR RESEARCH IN EYE MOVEMENT

(3) writing table. These are illustrated in Fig. 1. This equipment was developed from the need for an easy and effective means of presenting advertising copy to the reader, in connection with investigations on the attention-value of advertising.

The exposure-device has been employed for the presentation of a series of advertisements, and when used in connection with a tachistoscope (time-interval control device), such factors in the study of advertising, as, what sort of material first attracts the attention, and what material attracts the most attention, may be investigated. For example, one may undertake to arrange a number of advertisements in their rank order of merit, from most attractive to least attractive. Such a study may be made by pairing each in the series of ads with every other ad in the series, and exposing each pair to the attention of the reader for a definite interval of time. Once such a series has been so constructed, it may be used as a basis for determining the attention-value of any other ad it may be of interest to investigate. To evaluate a new ad on this basis, one need simply determine which ad in the standardized series a number of observers find to be equally attractive.

To prepare advertising copy for use, the ads are mounted on a series of rectangular cardboards and stacked in the order in which they are to be presented. The cards are then hung from the top, on a pin, and will be seen in the proper order as they are fed through the exposure device. The cards are fed through by pressing a plunger on the back of the panel, causing the front card, or pair of cards, to drop into the box below, and presenting the second card, or pair of cards, to the attention of the reader. If the room is darkened between one presentation and the next, and if the reading lights are controlled by the tachistoscope, each successive pair may be exposed for a predetermined interval of time.

A photographic record of the eye movements of the reader, taken while he is observing the material as it has been presented above, is considered to be objective

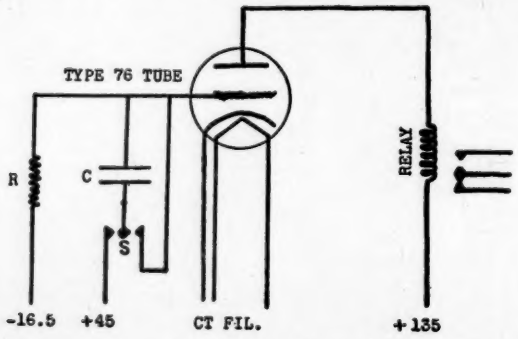


FIG. 2. DIAGRAM OF THE FUNDAMENTAL CIRCUIT OF THE TACHISTOSCOPE

evidence on the attention-value of whatever copy is under investigation. On the basis of such an eye movement record, this series of ads may be arranged in their order of merit. In making any such photographic record, both lights and camera are controlled by the tachistoscope. While the master switch on the panel of the tachistoscope is used to control both reading lights and camera, the tachistoscope, in reality, controls the length of time both lights and camera are on; the operator merely determines when the interval is to begin, and does so by closing the master switch. The tachistoscope darkens the room and stops the camera after a definite interval of time has elapsed. The range of time intervals available on our tachistoscope is from 0.1 to 15 sec., and the interval to be used is a matter of choice. After any given interval has been chosen by an appropriate setting of the time-control switches, the time-interval during which lights and camera are on may be repeated time after time, wholly by means of the master switch. This master switch on the panel of the tachistoscope, is in parallel with a remote control switch, the use of which allows the experimenter considerable freedom of movement during an experiment, independently of wherever the tachistoscope may have been placed in relation to the other equipment.

The writing table is for the convenience of the reader whenever he may be required to make any sort of written response in connection with the material he is

reading, as, for example, in studies on problem solving. This table has been so constructed as to change the paper at the press of a button placed at the foot of the reader, permitting him to make a change in the paper without diverting his attention from the problem at hand. Here, too, the tachistoscope has been so arranged as to control the change in paper.

For the information of those who may be interested in constructing similar equipment, a more thorough explanation follows:

(1) Exposure device. The cards, on which the advertisements are mounted that are to be presented for a study of their comparative attention-values, are hung in the device on a pin inserted through eyelets placed in the top edge of every one. Blank cards hung in front of the first pair prevent the reader from seeing any copy until the experimenter is ready to begin. After darkening the room, a plunger at the back of the device is used to release the two blank cards in front, which then drop through a chute into the box below. The first pair of ads are then in a position to be seen for an interval of time controlled by the experimenter. By means of either the master or remote control switch, the tachistoscope is used to start the camera, flash the lights on for a definite time interval, flash them off, and stop the camera. The experimenter makes ready to present the next pair of ads to the reader, while the room is dark, by dropping the front pair into the box below, and resetting the tachistoscope.

Only one card at a time is released from the front end of each pin because of the small clearance between the pin ends and a stop extending across the front top edge of the cards. As the pins are adjustable in length, this clearance is just sufficient to release the cards one at a time when the plunger at the back of the panel is pressed forward. To be effective, the cards must be of uniform thickness, smooth and flat.

(2) Tachistoscope with remote control switch. Basically the operation of the tachistoscope (see Fig. 2)[1] depends upon reducing, for a short interval of time, the negative bias on the grid of a three-electrode vacuum tube, during which time-interval, the flow of plate current actuates a relay, temporarily completing an external circuit. The length of time during which the external circuit is closed depends upon how long the grid bias is insufficient to overcome the flow of plate current through the tube. This interval of time depends upon the size of condensor (C) and leakage resistor (R) in the grid circuit. By means of a single-pole, double-throw switch (S), a positive charge (45 volts) placed on one plate of condensor (C) attracts to the other plate, a negative charge which must be withdrawn from the grid, allowing plate current to flow until a sufficient charge of electrons from the power supply can flow through the leakage resistor (R) to re-establish the negative bias on the grid. The choice of time intervals, then, is merely a matter of selecting the proper size of resistor (R) and condensor (C) to be used in the grid circuit of the vacuum tube. Tap switches for the purpose of choosing any combination of a number of resistors and condensors, then, will serve to select any timed-interval within the range of values available. To repeat any given timed-interval, switch (S) is used to reset the circuit, by short-circuiting the condensor.

Based on the above principle, a multiple tachistoscope (see circuit diagram;

[1] Acknowledgement is made to Mr. Paul Griffith of the State University of Iowa for suggesting this fundamental circuit.

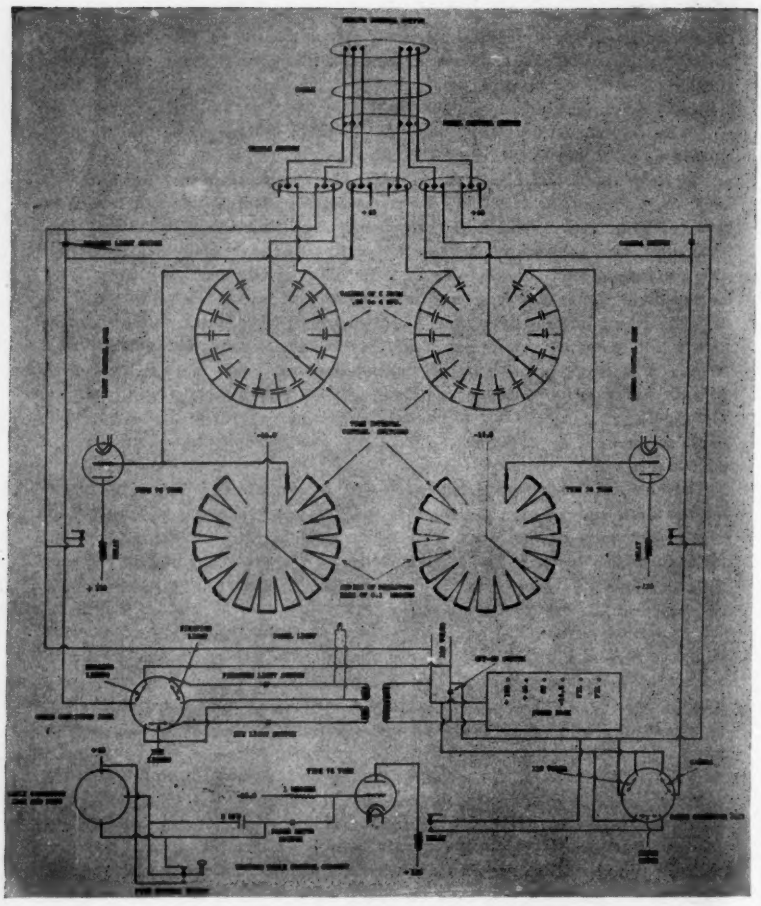


FIG. 3. CIRCUIT DIAGRAM OF THE ELECTRONIC DEVICE FOR TIMING THE OPERATION OF CAMERA, READING LIGHTS, AND WRITING TABLE

Fig. 3) was constructed for simultaneously controlling both the camera and reading lights. In this tachistoscope is a central switchboard consisting of a triple switch, actually three double-pole, double-throw switches. The combination of elements, and the way in which they are to be arranged for single-switch control, is determined by the position of these three double-pole, double-throw switches. Single-switch control is obtained with either one of the two switches marked panel con-

trol switch or remote control switch on the circuit diagram. One or the other of these master switches enables the operator to control any combination of lights and camera from anywhere within a distance of 15 feet, either tachistoscopically or non-tachistoscopically, at will, and to reset the tachistoscope for repeating the timed-interval. The range of timed-intervals with the values of R and C shown on the circuit diagram is, by steps, from 0.1 to 15 sec.

Incorporated within the same unit is a similar circuit for timing the operation of a motor used to replace the paper on the writing table.

(3) Writing table. In this device, a roll of 7-in. adding machine paper is threaded out through a set of sponge rubber rollers over a pane of glass set flush with the table top, and drawn on over through a second set of motor driven rollers. A light box beneath the pane of glass illuminates the writing surface from below. The foot control button indirectly closes the circuit through the motor drive for a period of time controlled by the tachistoscope. Releasing the foot button resets the tachistoscope for the next occasion.

As has been indicated in the foregoing discussion, this equipment is particularly well adapted to the study of consumer interest in advertising. It seems quite possible, however, that it might be found of value in various other types of research problems, such as the study of reaction times in response to various visual stimuli, as a simple substitute for the metronoscope in increasing the speed of reading, or in any type of research employing the presentation of a visual stimulus.

A NEW OBSTRUCTION BOX

By G. L. FREEMAN, Northwestern University

In the usual type of obstruction box the animal is re-introduced to the starting compartment after each successful crossing.[1] Such handling may very well interfere with goal orientation and so effect behavior measurements. Furthermore, the fact that only one type of obstruction (shock) is readily used does not commend the method for studies of frustration in which barriers of varied type are needed. A new obstruction box now in use in our laboratory seems to meet these difficulties and to present certain other advantages for the study of frustration and motivation in rats.

The apparatus (Fig. 1) consists of a continuous runway, divided into eight segments and arranged so that obstructing barriers may be introduced into each segment. The goal object (food or mate) is placed in a basket and moved around the runway somewhat after the fashion of the mechanical 'rabbit' in whippet racing. After systematic deprivation, the animal is introduced in a segment of the runway and allowed to connect with the goal object at a 'choice-point.' Then the basket is quickly withdrawn and moved across a 'barrier' by the experimenter, who stands in the center of the apparatus. In order to make further contact with the goal, the animal must now leave the choice-point and pass through the barrier. When such a

[1] F. A. Moss, A study of animal drives, *J. Exper. Psychol.*, 7, 1924, 165-185; C. J. Warden, The relative strength of the primary drives in the white rat, *Ped. Sem.*, 41, 1932, 16-33.

reaction is made the animal is again allowed a momentary connection before the basket is moved ahead. Segments of the apparatus lying behind each new choice-points are closed off so that the animal is always presented with the alternatives

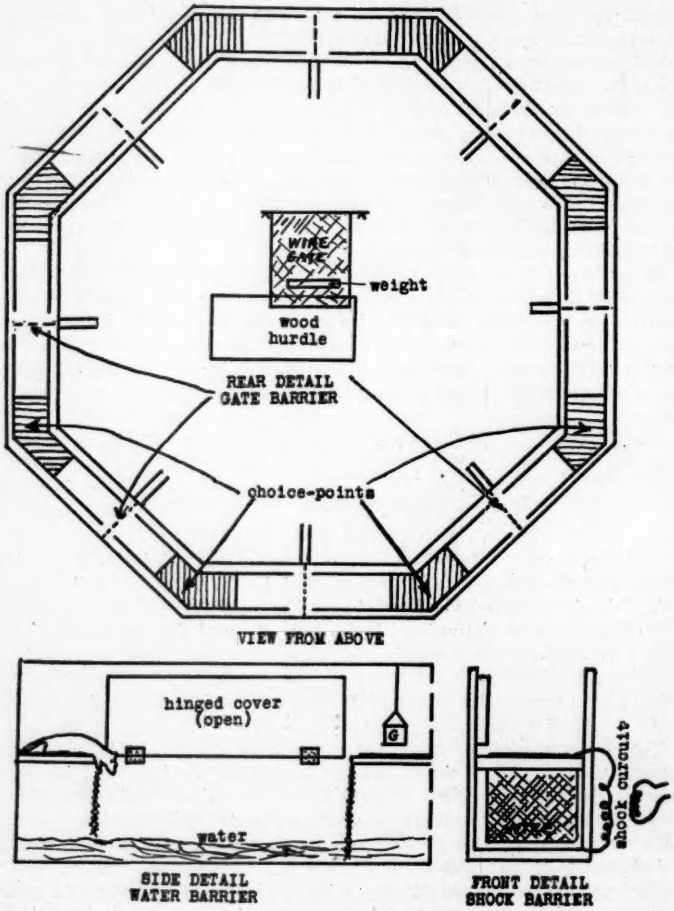


FIG. 1. A NEW OBSTRUCTION BOX

of advancing against obstruction in order to achieve a goal or remaining at the choice-point. Several types of barrier are available: (1) a continuous galvanized steel trough running under the choice-points is filled with *water* of constant tempera-

ture; (2) the trough may be drained and an *electric shock* delivered to the animal as he starts from a choice-point down into the now empty trough; (3) hinged covers may be dropped over the exposed part of the trough and weighted *wire gate* barriers placed between choice-points. Barrier as well as goal object is not changed in a given test period.

In actual practice if an animal stays at a choice-point or refuses to cross the barrier within an arbitrary time limit of 2 min., the goal object is again momentarily presented and withdrawn. This makes it possible to put the animal, technically at least, through a fixed number of units each day. Since the total number of units crossed has been found to correlate highly with average time spent per unit, we have usually chosen to end a day's run at the eighth unit and to permit full connection with the goal therein; animals who do not actually cross the entire eight units are not so rewarded before removal to the home cage. A few preliminary runs against a barrier at minimal potency are necessary to make the animal goal-oriented. When systematic experimentation begins the barrier is advanced to maximal potency.

By use of this apparatus it is possible to obtain several quantitative measures of behavior. These include: (1) time spent in obstruction unit (average and m.v.); (2) number of physical contacts with the barrier; (3) amount of vacillatory activity at a choice-point (turnings, tentative approaches); (4) number of regressions (refusal to advance upon the barrier or to maintain goal-orientation); and (5) eliminative behavior. It is also possible to obtain at choice-points measures of galvanic skin resistance by an adaptation of the potentiometer technique described by Muenzinger and Mize.[2]

APPARATUS NOTES

A Modification of the Darrow Behavior Research Photopolygraph

The Darrow Behavior Research Photopolygraph[3] is designed to record various physiological phenomena on a roll of photographic paper 175 ft. long and 6 cm. wide. The paper recommended by the manufacturers is P.M.C. normal white glossy bromide paper no. 1,[4] wound on core no. 93. At the current price of $3.15 per roll, this paper costs 1.8¢ per ft. A substantial reduction in the cost of operating the photopolygraph can be effected by slightly modifying the camera so as to permit the use of Insurance Bromide paper, Grade A,[5] which at the current price of $2.65 per 250-ft. roll, costs only 1.06¢ per ft. Insurance Bromide is 2⅜ in. wide, wound on core no. 1.

In order to convert the photopolygraph for the use of the slightly wider Insurance Bromide paper, it is necessary to widen the driving mechanism. This may be done by inserting a no. 8 brass washer at the end of each of the spacers of the driving mechanism. The bearing-plate which is thus moved from its former position is then soldered to the two pieces which form the slit of the camera. To remove the driving mechanism from the camera, detach the upper gear from its shaft and

[2] K. F. Muenzinger and R. H. Mize, The sensitivity of the white rat to electric shock: threshold and skin resistance, *J. Comp. Psychol.*, 15, 1933, 139-148.

[3] Darrow, C. W., The behavior research polygraph, *J. Gen. Psychol.*, 7, 1932, 215-219.

[4] C. H. Stoelting Co., Chicago, Illinois.

[5] Manufactured by Eastman Kodak Co., Rochester, New York.

remove the two screws which are directly above the gear train. The driving mechanism can then be slid out from the other side of the box.

Since the Insurance Bromide is thinner than the P.M.C. Bromide, the large removable roller should be covered with a piece of latex pneumograph tube in order to prevent the small driving roller from slipping on the paper. A strip of felt glued into the groove in the inner side of the lid of the metal paper magazine serves to provide room for the wider roll of paper and also maintains a light-proof joint.

The Insurance Bromide is a more sensitive paper than the P.M.C. Bromide. It is thus necessary to replace the red glass in the side of the photopolygraph with a piece of opaque material. Furthermore, a 15-c.p. single contact lamp should be substituted for the 32-c.p. lamp with which the apparatus is equipped.

The following developer has been used satisfactorily with the P.M.C. Bromide paper exposed by a 32-c.p. lamp:

Potassium bromide	0.5 gm.
Elon	1.5 gm.
Hydroquinone	5.8 gm.
Sodium sulphite	19.4 gm.
Sodium carbonate	38.9 gm.
Water	40.0 oz.

In using the Insurance Bromide at a speed of 4 mm. per sec. with the 15-c.p. lamp, this developer should be mixed with an equal amount of water. At a speed of 2 mm. per sec., one part of developer to 2 parts of water is satisfactory.

University of Missouri FREDERICK A. COURTS

A MODIFIED GALTON BAR

Judgments made on the Galton bar ordinarily found in the laboratory are frequently affected by visual cues on the face of the apparatus, *e.g.* flecks of dust, scratches, dents, and paint-blemishes. The involuntary or deliberate use of those cues introduce a source of error into the *O*s' judgments which may render the measurements invalid. We have, therefore, adopted in our laboratory a modification of the apparatus—first proposed and successfully used by Hamilton in 1929[1]—which eliminates all of those cues. Because Hamilton's modification is not generally known, we present here the essential features of our modification.

The usual bar-apparatus is placed in a large light-proof box into which *O* looks through an eye-piece. The standard and variable bars are adjustable slits mounted in the rear of the box. The slits are backed with a large piece of frosted glass, which diffuses the illumination—a bank of lamps, a Lumiline, or a north window. On looking through the eyepiece *O* sees two bars of light against a black background—no details of which are visable. Judgment of relative extent is, consequently, based solely on the apparent length of the two bars.

The instrument used by us consists of a box of 60 x 60 x 25 cm., painted black on the inside. *O*'s eyepiece is taken from a stereoscope. The two slits are 1.5 x 30 cm. and are covered by black metal shutters sliding in grooves fastened to the outside of the box. The slit on the left is located 2.5 cm. below that on the right,

[1] H. C. Hamilton, The effect of incentives on accuracy of discrimination measured on the Galton bar, *Arch. Psychol.*, 16, 1929, (no. 103), 1-73.

but there is no vertical overlapping until both slits are open to an extent exceeding 25 cm. The shutters are adjusted by means of a strong string attached to them at a point above or below the slit (so as not to appear in the illuminated bar) and extending it in a loop between two large pulleys at the sides of the box. Tension on the string is maintained by a small spring. The pulley to *O*'s right is attached by means of a flexible coupling to a shaft carrying the control knob. The apparatus is so arranged that either shutter may be used as the variable and the standard by a simple alteration of the string attachments. This permits a change of space order. Pointers, attached to the shutters, move along millimeter scales. Variable electrical contacts may be provided on the scales and pointers for producing sound or light signals, if desired, to indicate a given error of judgment.

College of William and Mary R. H. HENNEMAN and J. C. TALLEY

NOTES AND DISCUSSIONS

Correlations Between the University-Grades of Parents and Children and of Siblings

This is a study of the correlation between the standing in the University of Michigan between parents and children and between siblings, for the light it may throw upon inheritance of intelligence.[1] Three previous statistical approaches to the problem have been made. The first was Pearson's determination of the coefficient of correlation between siblings based on the estimates of teachers.[2] His frequently quoted conclusion was that intelligence and other mental characteristics showed a correlation coefficient of about 0.50. This is approximately the same as the correlation for physical measurements.

A second study was made by Shuster and Elderton of the relative standing over a period of 92 years of fathers and sons in the Oxford University honors examination.[3] The honors examination is a test extending over four days and is reputedly one of the most severe academic tests applied anywhere. The results use only five ranks: first, second, and third class honors and passed and no degrees. The grades are sharply marked off at the top but group the great mass into only two categories. Slightly more than a third obtain honors against nearly two thirds in the pass degree and no degree group. The coefficient of correlation between fathers and sons was 0.312, and between brother and brother about 0.40, from 0.39 to 0.42 by different methods of calculation.

A somewhat similar study of the boys in Harrow and Charterhouse indicates correlation of about the same amount for brother-brother relationship. Here the ranking in the class for one term was taken as the basis of comparison. The average coefficient of correlation for all groups was 0.398, a very close agreement with the Oxford result. No values were obtained for the father-son relationship as the records did not give the names of the fathers. In an appendix to the article,[4] Karl Pearson states that all such correlations from academic groups are too low because the individuals who attend colleges are selected from a small section of the population. He developed a formula which made possible the calculation of the true correlation from the rough one on the assumption that the variability is reduced through self selection by a definite percentage. Applying this formula to the Oxford and Charterhouse studies he finds that the correlation should be 0.44, if the distribution is reduced 15% and 0.54, if it is reduced 25%. Whether the reduction is one or the

[1] Thanks are due to the National Youth Administration, and to the Rackham Fund of the University of Michigan for funds to carry on the research. I desire to thank Professor George Meyer for asking his class in statistics to compute the correlations for the siblings, and Mr. Forth, Mr. Bussell, and Mr. Bond of the National Youth Administration for collecting the data and for much help in computations. I desire especially to thank Miss Ann Sylvester for checking the computations of the others and for many computations of her own.

[2] Karl Pearson, Inheritance of mental and normal characteristics in man, *Biometrika*, 3, 1904, 131-190.

[3] Edgar Shuster and E. M. Elderton, The inheritance of ability, *Eugenics Laboratory Memoirs*, 1, 1907, 3-42.

[4] Karl Pearson, Influence of academic selection on correlation coefficients, *Eugenics Laboratory Memoirs*, 1, 1907, 41-42.

other of the percentage can hardly be regarded as proven, but there is certainly some selection so the coefficient should probably be greater than it appears on the face of the returns. This brings the correlations to the neighborhood of 0.50 which Pearson's students have regarded as normal since his first paper.

We need not summarize the results of the studies of twins and ordinary siblings. The method was different in that the measurements were by tests, the subjects were of all ages, and entirely unselected from the community. It will be recalled that, according to Newman and his collaborators,[5] identical twins show correlations of 0.90 or more, fraternal twins from 60-80 and ordinary siblings of 0.50-54.

All of the studies agree in showing considerable similarity between father and child and brothers and sisters. Our investigation takes a form more like the study of Shuster and Elderton. We also dealt with school grades and calculated the correlations both between parents and children and between siblings of the same family. The fathers and mothers of students in college at the time of the investigation were traced from answers to a question on the blank filled in by entering students that asked where and when a parent attended college. These were confirmed by the records. We made three different studies of correlations between parent and child. In the first two cases the parents had attended the university before the present system of using letters for designating standing had been introduced.

Down to 1912 the only grades recorded were passed, conditioned, and failed. The conditioned students were given a second examination. This mark indicates the mediocre students in the particular course. We used a system suggested by Mr. Bussell, one of our N.Y.A. workers, to assign numerical values to these rough designations. A record of 9 was assigned arbitrarily to all students who took just the required number of courses and passed each of them. The others had a point subtracted for each course failed and a half-point for each course in which they were conditioned. Students who took more than the required number of courses were given added credits. There were few of these. In our first study of 294 pairs, the grades grouped themselves rather narrowly. Forty-nine had a grade of 9 and only 12 fell below 5.

The children all attended while the usual letter grading was in vogue. For these we assigned a value of 4 to A; 3 to a B; 2 to a C; 1 to a D, and 0 to an E. The sum of these points is called the honor point record, and what is known as the honor point ratio is the quotient of the sum of the honor points divided by the total number of hours taken. Our scheme for changing the older passed, conditioned, and failed marks into numerical values lumps all the best individuals into one group. This included approximately a sixth of our first group. The Oxford studies on the other hand distinguished the best fairly sharply, into at least three groups, while it lumped the great mass into only two categories. The letter grades made possible very close indications of standing for the best and the worst.

Two studies were made at different times comparing parents graded on the old system with children graded on the new. The first correlated 294 pairs of parents and children and gave the very low correlation of $0.05 \pm .038$ A second group of 112 different pairs gave the higher correlation of $0.17 \pm .062$. No satisfactory explanation can be given as to the large difference. The span in years between

[5] H. H. Newman, F. N. Freeman, and K. J. Holzinger, *Twins: A Study of Heredity and Environment*, 1937, 55-93.

attendance of the parents in the two groups was a little greater for the first group. That, however, should not be an important factor. There were rather more required courses in the period when some of the parents attended the university. That might have made a difference but should have worked to the disadvantage of all parents alike, and so should not have affected the correlations.

The wide divergence in the two coefficients and the question as to the accuracy with which our arbitrary transfer of passed, conditioned, and failed into numerical values represented actual differences in ability led us to undertake a third study. The individuals in this study were all made up of pairs in which the parents attended after the new marking system was put into effect. Both parents and children were rated by honor points, with its greater fineness of discrimination at all levels of excellence. In this third group, as in the earlier, where two children of the same family were found the average honor point ratio for the two was used in the computation. This method of averaging the standard seems preferable to using each child separately as did Shuster and Elderton or to omit one child as did Dr. Gordon. To omit one might lead to yielding to the temptation of selecting the one that would favor a desired result, if any result was desired.

The third group consisted of 123 pairs. The coefficient of correlation was 0.23 $\pm$ .057. This result is obviously more reliable than either of the other two. The measures of standing are more accurate and more consistent for parent and child. The time interval between parent and child was less than in the other two studies, at the greatest a difference of twenty-six years. None of the parents were in college at the time the required course system was effective. There would also be less difference in the organization of courses and in other imponderables of attitude of students and of the personnel of the instructing force. If we should average the three correlations without considering the number of cases, the result is a coefficient of 0.15. A better idea of the result would be given by saying that it ranged in three groups from 0.05 to 0.23. It would probably give a truer picture if we considered only the last determination, obtained under the best conditions. The latter value shows a significant if slight relationship, the former none at all. It may be worth while to indicate that all are positive.

A study of the correlation in standing between siblings of the same family did not meet with the difficulties encountered by those of parents and children. All siblings studied were in attendance within comparatively few years. The methods of grading were the same and the social and educational conditions did not vary appreciably. First a coefficient of correlation was obtained for all pairs without reference to sex. This was 0.25 $\pm$ .05 for the 175 pairs used. Then the data were broken down with reference to sex and the three possible relationships: brother-brother, sister-sister, and brother-sister were calculated separately. Wide divergences were found for the different combinations. For 85 cases of brother-brother combinations, the correlation was 0.149. The 26 cases of sister-sister pairings gave a coefficient of 0.565, and 63 cases of brother-sister groupings showed a coefficient of 0.464. One might ask why the values were so different between the groupings. It is easier to ask than to answer. There are no obvious reasons why sisters should be more alike than brothers, or why a boy should be more like a sister than a brother. In the latter case one would expect the relations to be reversed. Probably the differences must be charged to chance, due to an insufficient number of cases, when they were divided into the smaller categories.

It might be mentioned that an earlier investigator, Elderton,* found similar discrepancies between different pairings based on sex, but the differences did not in all cases run in the same direction as ours. The results were derived from studies of brothers and sisters in two English elementary schools. Miss Elderton does not say how many were studied, but is apologetic for the small numbers. Two methods of evaluating intelligence were used: the Stanford-Binet and estimates by teachers. Corrections were made for differences in age which were believed necessary because the brighter children in the upper grades had been promoted. The correlation between brother and brother was 0.489 when the I.Q. was used as a measure and 0.486 when the teachers' estimates were used. The sister-sister correlations were 0.384 and 0.540 for the two methods of measurement. The brother-sister correlations were 0.532 and 0.499 respectively. The differences are much less than those we found and differ in direction. For her the brother-sister correlations were highest while in our results they were intermediate to the very high sister-sister correlation and the very low brother-brother. From the small number of cases available in either study, one can hardly draw conclusions that would hold as to the general hereditary resemblances between the sexes, even were the results consistent in the two studies.

As compared with the Galton Laboratory work our correlations are all considerably lower. The father-son relationship for Oxford honor students gave a coefficient of 0.312, our parent and child correlation was 0.23 for the most favorable conditions. The sibling coefficient (brother-brother) at Oxford and for the public schools was 0.39 to 0.42 and 0.398 respectively, ours was 0.25. We could argue on Pearson's assumption of restricted selection from the population of students that the true correlation would be ten points or more higher. Since any estimate as to how great the selection is, is purely arbitrary, all calculations would be of little value. We can rest content with the statement that they could be made higher if selection is taken into account. An explanation of the difference may lie in the more comprehensive material involved in the grades in the two cases. The Oxford honors examination is a comprehensive test covering a relatively limited class of material that is approximately the same for all examinees. The grades given in an American university cover very diverse material, with somewhat changing standards of marking for the same course at different times and for different courses. The smaller correlation may well be due to the diversity of the material and of the scoring. The lower correlations obtained when tests are used as a measure rather than college standings of all sorts would also be due to the restricted material of the test and to a smaller degree of selection among the individuals tested. Moreover, intelligence is probably only one of several factors in determining college standing. In addition, persistence, confidence in ability and many other emotional and volitional qualities contribute to determine the accomplishment and the grade that will be given. The test requires ability to work intensively for a brief period, while success in college demands continued effort and often without immediate incentive. These other factors are also probably inherited, but not necessarily in the same degree.

If one considers only the higher correlations and makes an allowance for selection, the correlations appear fairly significant. The correlation between brothers and sisters is not so much lower than between the results of mental tests and standing

* E. M. Elderton, Present position in regard to the inheritance of intelligence, *Biometrika*, 14, 1923, 398 ff.

in college or than between standing in lower schools and standing in college. If one chose students on the basis of the standing of a brother or sister one would not do so much worse than to choose by tests or by high school grades. Siblings give a closer correlation because both have the same parents, while the correlations between parent and child consider only one parent.

University of Michigan W. B. PILLSBURY

A NOTE ON THE USE OF "OMNIBUS" TRAINING TO VALIDATE SEASHORE'S "CAPACITY" HYPOTHESIS

Many investigators have concerned themselves with the validity of the Seashore tests, using as their criteria of validity grades in applied music and theory courses. These investigations have in most instances indicated relatively low coefficients of correlation.[1] Against this procedure Professor Seashore has vehemently protested, maintaining that the tests "should not be validated in terms of their showing on an omnibus theory or blanket rating against all musical behavior, including such diverse and largely unrelated situations as composition, directing, voice, piano, violin, saxophone, theory, administration or drums; because there are hundreds of other factors which help to determine the job analysis in each of such fields."[2] Seashore further states that "I have been bombarded all these years by the omnibusists for this type of validation, but I have persistently refused action on the ground that it had little or no significance."[3] The tests, he says, "represent the theory of specific measurements insofar as they conform to the two universal scientific sanctions, on the basis of which they were designed; namely, that (1) the factor under consideration must be isolated in order that we may know exactly what it is that we are measuring, and that (2) the conclusion must be limited to the factors under control."[4]

A correlative assumption in Seashore's psychological theory is that training has no significant effect upon scores in the Measures of Musical Talent. This is one of the fundamental postulates of the "capacity" hypothesis.[5] In this connection, Stanton's studies on the effects of musical training upon talent scores have been approved and sponsored by Seashore and have been accepted generally.[6] In his preface to one of Stanton's studies, Professor Seashore writes:

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[1] P. R. Farnsworth, An historical, critical, and experimental study of the Seashore-Kwalwasser test battery, *Genet. Psychol. Monog.*, 9, 1931, 291-391; J. L. Mursell, What about music tests?, *Mus. Ed. J.*, 24, 1937 (Oct.), 16. The issues of the test discussion, *ibid.*, 24, 1938 (May), 22.

[2] C. E. Seashore, Two types of attitude toward the evaluation of musical talent, *ibid.*, 24, 1937 (Dec.), 25.

[3] *Op. cit.*, 26.

[4] *Op. cit.*, 25.

[5] A paper on "The present status of the 'capacity' hypothesis in Measures of Musical Talent" is now in preparation. This paper will attempt to classify, summarize and evaluate the literature dealing with the question of the non-improvability of scores in the Seashore Measures through training.

[6] H. M. Stanton, and W. Koerth, Musical capacity measures of adults repeated after musical education, *Univ. Ia. Stud., Aims & Prog. Research*, 31, 1930, 1-18; Musical capacity measures of children repeated after musical education, *ibid.*, 42, 1933, 1-48; H. M. Stanton, Measurement of musical talent, *Univ. Ia. Stud. Psychol. of Music*, 2, 1935, 1-140.

The Measures of Musical Talent here discussed were built on the assumption that they should measure these specific capacities before musical education was begun and that the capacities would not be greatly modified by training. Attempts to validate this assumption have been made by various indirect methods, but the final critical proof has awaited the accumulation of successive measurement upon the same individuals before, during, and after musical training. This has been accomplished through the accumulation of a remarkable series of measurements by Professor Stanton.[7]

Seashore apparently relies upon such data as Stanton's to substantiate his claim that training does not materially improve test scores, and that the tests are, therefore, stable 'capacity' measures, on the basis of which predictions of future scores may safely be made.

The correctness of these views must in large measure depend upon the appropriateness of the training employed. It is, therefore, pertinent to inquire into the type of training used by Stanton. This consisted of (1) individual lessons in voice and instrument—piano, violin, 'cello, harp, organ, flute, tympani, (2) group training in orchestra and chorus, and (3) theoretical subjects such as harmony, counterpoint, appreciation, theory and conducting—the typical 'omnibus' situations, 'diverse and unrelated,' which were rejected by Seashore for *validation* of the tests!

If the tests cannot be validated against 'omnibus' criteria, how can the failure of such training to produce significant increases in scores be said to prove that scores in the tests are not improved by training of any kind? If the only criteria for validation are "specific" and "internal," *e.g.* correlation of the Sense of Pitch test with "objective records of musical performance in pitch intonation or ability to hear artistic pitch deviation in the musical situation," then may not also "specific" training, closely related to the content of the tests, be requisite for proof of the assumption that the test scores are unaffected by training? Obviously, it is logically possible that certain "omnibus" training procedures may transfer in some degree to a "specific" test, but the real question is whether anything short of intensive training which is purposefully remedial and directed toward improving a "specific" capacity, can be said to demonstrate conclusively that training is immaterial. That it is possible to develop such specific training methods seems indicated by Wolner's study of the effects of training upon pitch-deficient children.[8]

Assuming that the Seashore tests are valid, and accepting Seashore's criticism of the use of "omnibus" criteria to challenge the validity of his tests, it seems proper

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[7] Stanton and Koerth, *op. cit.*, 42, 1933, 3. Stanton's method was to give the tests to students entering the Eastman School of Music and to retest after a 3-yr. interim of formal musical education. The absence of significant improvement in scores upon retest is the "final critical proof" referred to above.

[8] Wolner gave intensive individual training in vocal reproduction of intervals and in pitch discrimination to 7 children who were so markedly pitch deficient that they were not only unable to discriminate tuning fork differences of 30 ~ (at a standard of 435 ~), but were incapable of distinguishing piano intervals as large as an octave. Each child was given 20 min. of training daily, 5 days a week for a period of 81 days. Results in part: 4 out of 7 of the children reduced their thresholds in pitch discrimination to 0.5 ~; the remaining 3 had thresholds of 2, 3, and 5 ~ respectively. (Cf. M. Wolner, An experiment in individual training of pitch-deficient children, Unpublished Master's dissertation, Colleges of the City of Detroit, 1932; M. Wolner, and W. H. Pyle, An experiment in individual training of pitch-deficient children, *J. Educ. Psychol.*, 24, 1933, 602-608.

to question the use of the 'omnibus' type of training to demonstrate the non-improvability of 'capacity' scores.

Northwestern University RUTH WYATT

A SIMPLE MEANS OF PRESENTING AMBIGUOUS VISUAL MATERIAL POSSESSING COLOR, DEPTH, AND MOVEMENT

The success of the Rorschach test has called attention to the valuable diagnostic material that becomes available when the individual is told to organize and interpret relatively ambiguous sensory materials. Stern's[1] development of cloud pictures and Skinner's[2] use of the verbal summator are other recent examples of the same general type. This note describes a simple means of presenting ambiguous visual material which possesses color, depth, and movement, and which may be interpreted in meaningful fashion by an observer.

The principle involves the passage of polarized light through cellophane. If white light, which vibrates in heterogeneous directions, is passed through a polarizer it is then given a uniform vibration direction. Upon its further passage through a layer of cellophane, which acts as a double refracting medium, the light becomes elliptically polarized, *i.e.*, the vibration directions fall in a continuous ellipse. The constants of the ellipses are different for each wave-length composing the white light. If this elliptically polarized light is then passed through a second polarizing medium, there is a differential absorption which is determined by the constants of the ellipses and hence is different for each wave-length. The original white light will then become colored since the rays of different wave-lengths are being differentially absorbed.

These are interference colors, and a subtractive spectrum is available. The spectrum is a first-order interference spectrum of gray, white, yellow, orange, red, purple, blue, and green. Since the absorption is determined by the ellipticity for a ray of any given wave-length, and since this ellipticity is a function of the thickness of the double refracting medium, the original color will be determined by the thickness of the cellophane.

If the second polarizer, or analyzer, is rotated through 90°, the rays fully transmitted at first will now be fully absorbed, and those previously absorbed will be transmitted. There will be a corresponding reversal from transmission to absorption for the rays of those wave-lengths partly transmitted. Therefore, as the analyzer is rotated, the original color will pass through a stage of desaturation and then change to its complementary. The color thus is a function first of the thickness of the double refracting medium and secondly of the angle of rotation of the second polarizer.

These principles may be applied specifically as follows: White light from a concentrated light source (electric light bulb) is passed through a polarizing screen consisting of either a celluloid film (Polaroid type 2) or of a celluloid film laminated between two layers of glass (Polaroid type 1). These are supplied by the Polaroid Corporation. This polarized light is then passed through cellophane. One

[1] W. Stern, Cloud pictures: A new method for testing imagination, *Character & Pers.*, 6, 1937, 132-146.

[2] B. F. Skinner, The verbal summator and a method for the study of latent speech, *J. Psychol.*, 2, 1936, 71-107.

must be careful to use cellophane and not kodapak or protectoid, as these last do not have the requisite molecular orientation. To hold the cellophane securely it may be fitted into the interior of a section of cardboard packing tube. If the fit is a snug one, the sides of the tube will hold the cellophane in place. If one now places a second polarizing screen against the end of the tube and looks through this lens at the cellophane within the tube, one sees a varicolored, patterned field not unlike the field of a kaleidoscope, except that it has depth.

The colors are determined both by the thickness of the individual sheets of cellophane and by the rotation of the second polarizer. The thickness of the cellophane may be varied by superposing individual layers on each other. These may be stuck together with ordinary rubber cement, or merely placed at different depths in the tube. Patterns may be worked out by varying the number of layers of cellophane in different parts of the field. One may get an excellent random effect by simply crumpling up the cellophane into a ball and placing it in the tube. By wetting the cellophane and stretching it in spots, distortion of the molecular orientation is caused in local areas with resulting color changes. Many color effects are possible.

Depth is present since one is looking at a three dimensional field, with the different layers of cellophane at different positions in the tube.

The semblance of movement may be obtained by rotating the second polarizing lens. This causes the colors in each part of the field to desaturate, pass into their complementaries, and finally reach their original values again. The resulting color effects resemble movements with the colors continually disappearing and reappearing in various portions of the field.

In one form of the apparatus, as set up by the writer, the parts of the apparatus were mounted in line in a wooden trough. The second polarizing screen was rotated by peripheral friction drive through a gear wheel connected to the shaft of a variable speed motor. The use of a rheostat enables one to control the speed of the color changes or 'movement.' Interchangeable lengths of cardboard tubing were used, so that many patterns would be available.

Preliminary investigation shows that the extra dimensions of color, movement, and depth may act as limiting factors, and that the interpretation is sometimes not as easy nor as varied as with the standard Rorschach ink blots. On the other hand, when interpretation does come it is often more 'vivid.' Many individual peculiarities of response are also noticeable. The method suggests itself as an interesting addition to present interpretative tests.

Wheaton College WILLIAM A. HUNT

BOOK REVIEWS

The Psychology of Art. By ROBERT MORRIS OGDEN. New York, Charles Scribner's Sons, 1938. Pp. vii, 291.

Professor Ogden may have had some difficulty in choosing a title for his book. The first part is called "Aesthetics"; the other four parts deal with the separate arts: music, poetry, visual arts, and eurythmics. By naming only the first part "Aesthetics" he has forestalled the possible criticism that much of what he has written is not esthetics. Whether the book as a whole is psychological is a more debatable question. Certainly the psychological treatment, a combination of response and Gestalt psychology, is in evidence throughout the book, as are also esthetic principles; but Ogden is particularly interested in "good" forms objectively given, and the emphasis—or "ground-rhythm" as he might say—is primarily upon the formal elements. A knowledge of these elements is undoubtedly of much value for the artist, but some readers, unconvinced by the argument that combinations of sound, line, etc., are good or bad in their own right, might wish that the author had devoted more space to explaining why the forms that are found in art happen to be acceptable. This belief in objective form has at any rate given the author the courage to pass dogmatic judgment upon the esthetic values of specific works of art, although he shows a commendable tolerance when discussing the modern trend.

The part on esthetics treats principally of the esthetic attitude, which the author believes should in the narrower sense be called "the artistic attitude," and of the classification of the arts. His description of the attitude is clearly stated in behavioral terms stripped of the traditional and vague philosophical implications which so often confuse the issue.

For Ogden there are two main types of behavior, active behavior which is either positive or negative—or as Holt would say, adient or abient—and passive, which may either be a state of quiescence due to inability through conflict to take an active attitude or passive contemplation and appreciation. All of these types are esthetic "in their primitive or instinctive modes of expression." This is obviously a very broad statement, which, however, he makes more specific when describing the esthetic attitude. This attitude is distinguished from the practical "give and take" attitude by the fact that we do something *with* rather than *toward* an object. The author states that the technical name for the attitude is empathy which, according to the reviewer, seems too general an application of that useful term.

Ogden is very much at home in the field of music and handles the subject as an expert. For the average readers who are interested more in general problems of esthetics than in special fields, he is perhaps too much at home and in consequence for them much of this chapter may be difficult of comprehension—as for example, his description of the melodic principle of the tonic trend: "All melodies composed in a scale of just intonation will indicate effects of finality on those notes which, in relation to preceding notes, reduce to the ratio-number 2 or a power of 2." As Ogden agrees that "music alone can give a sheer pattern," he devotes most of these chapters to the musical scale, harmony, counter-point, cadence and other items of musical composition. He also describes atonic music, folk music, and primitive music.

After explaining Schönberg's composition on the principle of equal semi-tone intervals and their multiples, he states that "Although untutored listeners cannot immediately 'hear' this logic, there appears to be no reason why they should not

learn to do so." One wonders how necessary it is to know this logic in order to appreciate Schönberg and whether, if such knowledge is obtained, the appreciation does not become fundamentally intellectual rather than esthetic. The same thing might be said of a knowledge of dynamic symmetry which will be mentioned later under the visual arts. In this connection Ogden admits, quite rightly, that the formal relations are usually apprehended naïvely, as pleasing or otherwise, without direct knowledge of the underlying geometrical forms. Even such simple mathematical relations as those in the musical scale may not be known by a person of keen musical appreciation. The reviewer remembers the day which an outstanding composer spent with him in order to learn about these tonal relations. Surprisingly his first question was: "What is meant by the octave being a relation of 1 to 2?"

In the chapter on poetry the author shows in a most interesting way how close is the relationship between form and meaning. As he correctly remarks, "behavioral impulses determine the meaning of words and phrases, and these find their expressive utterance in verbal forms, more or less suited to the intent of the mover." Most editors would, however, probably think that he is too optimistic when he states: "Any meaning which affects our being will demand a concise and simple way of saying it." These chapters contain much detail about poetic form with excellent illustrations, and there is also an interesting and important discussion of the relation of poetic thought to prose.

The part on visual art, which covers the broadest field, takes up almost half the book. The keynote is contained in the statement, "Starting with a canvas, a block of stone, or the area of a projection screen, the artist's problem is to design this given space with an enrichment of partial patterns which will improve and make more evident its unique and original wholeness." The first of these chapters, therefore, deals with the perception and representation of space. In his explanation of the third dimension the author places too much importance upon the act of convergence, which he uses even to explain the effect produced by the stereoscope. The chapter on color contains much that is found in an introductory psychology, including surface and film color, and color music.

The chapter on geometric design is concerned primarily with naïve geometry and dynamic symmetry which are familiar to us principally through the writings of Hambidge. The author soon has the reader thinking in terms of root-two rectangles, golden sectors, and whirling squares. That buildings like the Parthenon may be analyzed into geometrical designs cannot well be denied, and for those who are unacquainted with the intricate form of symmetry to be found in art, this chapter is valuable. But one often wonders in following the analysis of pictures, statues, and architecture in terms of dynamic and static symmetry how much is merely wish-thinking upon the part of the enthusiastic author and how much the actual intent of the artist.

In the chapter on painting and sculpture "certain influences of materials and techniques on drawing, painting and sculpture" are discussed. The important difference in the effect of free carving and modeling, and of drawing and painting, are clearly outlined. The development of modern art is also sympathetically presented.

The chapter on the techtonic arts, in addition to buildings, includes house furnishing, costumes, gardening and landscaping, staging and screen-presentation. The principles underlying the treatment of masses and the harmony of parts are

rather fully explained. The range of illustration is from the Temple of Neptune at Paestum, through Notre Dame of Paris and the Empire State Building, to modern streamlined dwellings and factories.

In the part on eurythmics the author takes occasion to impress upon the reader the importance of well-coördinated rhythmical movement, and lauds the efforts of Dalcroze to encourage the attainment of intricate, rhythmical responses. In the final paragraph of the book, in discussing the reciprocal adjustment between the organism and the environment, he makes the important statement that "The greater the degree of orderliness, the greater will be the efficiency of this adjustment. Less lost motion, and more enjoyment: this is the simple and practical precept of an esthetic method of education in all arts and all the skills."

The Psychology of Art is a valuable book. It contains much evidence of the erudition, sensitive esthetic appreciation and versatility of the author. Owing to the originality of style and the nature of the subject matter, the book is not easy reading and therefore probably not well-suited for the beginning student of esthetics or art. It will, however, undoubtedly prove helpful to the specialists in the various fields which are included in the book.

Princeton University H. S. LANGFELD

Psychology of Music. By CARL E. SEASHORE. New York, McGraw-Hill Book Co., Inc., 1938. Pp. xix, 408.

The motto of this book is said by Professor Seashore to be *"Not psychology but to psychologize."* That is, the book is intended to be a series of incomplete skirmishes upon various problems of music, designed to "stimulate and guide the student of music in scientific observation and reasoning about his art."

The problems discussed are both theoretical and applied. Under the first rubric may be included those chapters (A Musical Ornament, The Vibrato; Pitch; Loudness; Duration; Timbre; Tone Quality; Sonance; Consonance; Volume; Rhythm; Timbre of Band and Orchestral Instruments; Violin; Piano; Voice) dealing with the physical characteristics of renditions by voice and various instruments as recorded by oscillograph, piano-camera, and harmonic analyzer, and as displayed in the numerous 'performance scores,' tone spectra, and tables. These physical characteristics are related to the perceived tone; and the point is emphasized again and again that in musical art "all is illusion," *i.e.* what is heard is rarely, if ever, an exact duplication of the physical event. Other topics of psychological significance are considered, such as pitch discrimination, subjective tones, audible frequency ranges, audiometry, beats, and resonance, although at no time is a complete summary of facts attempted, and many times key definitions are completely omitted. Indeed, an outstanding quality of the book is its skeletal and selective structure.

Throughout, there is observable Seashore's deep concern with problems of application, which becomes explicit in his chapters on Learning in Music, Principles of Guidance in Music, Measures of Musical Talent, Analysis of Talent in a Music School, Analysis of Talent in the Public School, and The Development of Musical Skills. His program of application involves not merely the psychologist with his "Measures of Talent" test-scores, but also critical appraisal of many more data, such as motivation, family tradition, and willingness of the subject to work.

While these chapters on application are highly valuable, on the whole, the reader will stand aghast at the recommendation to "trust the first impression . . . instead

of repeating the impression, repeat the recall or memory," and at the blanket rule to learn by parts. The treatment of Thinking in Music, and Imagining in Music are unsatisfactory. "Thinking" resolves itself into a matter of I.Q., and involves a discussion of the questions "Are musicians, as a class, intelligent?" The chapter on imagining in music simply quotes various composers on the rôle of imagery in their own work, and does not attempt to go much further, except to state that imagery may operate in the hearing, recall, and creation of music. Seashore states that although imagery "is a phenomenon which does not lend itself to psychophysical measurements," he is inclined "to give it a central place and to expect the most immediate and helpful leads from an informal inventory of the use of mental imagery." But surely the student of remembering and imagining will have more to offer, even if only by way of a program of research, than a mere "informal inventory." Indeed, a general criticism is that Seashore seems to stop short with his quantitative data and simple report, never suggesting the possibility of laboratory investigation into the 'higher mental processes.' Again the reader may quarrel with Seashore's definition of rhythm as an "instinctive disposition," or his derivation of rhythmic measure from the dubious "attention wave."

The book has as a main theme the contention that the psychologist in music has two chief tasks: (a) the measurement of four physical variables (frequency, intensity, duration, and composition of the sound wave) and the determination of their psychological correlates under various conditions; and (b) the integration of facts from anatomy, physiology, physics, anthropology, and acoustics. The first of these problems is the main concern of the book; the second is resolved mainly by proclamation of the problem.

There are, in addition to the already mentioned topics, brief chapters on The Inheritance of Musical Talent, Primitive Music, and Musical Esthetics, and three preliminary chapters on The Musical Mind, The Musical Medium, and The Science of Music.

Under the hands of a competent and informed instructor, the book may be used to great profit with students having some slight acquaintance with the first principles of psychology. Its extended dissection of performance scores, alone, should yield the student some exciting knowledge and understanding of the possibilities of scientific investigations into the realm of music.

Cornell University JOHN IRVING LACEY

Grundlinien der Kunstpsychologie. By OTHMAR H. STERZINGER. Leipzig, Leykam-Verlag, 1938. Pp. xiv, 279.

The main purpose of Sterzinger's book is to establish some differentiation between experimental esthetics and experimental psychology in the analysis of art. On the hypothesis that the former is strictly normative, and the latter strictly factual, he has constructed a text of very systematic exploration of all the elementary sensory functions, perceptual processes, and subjective factors which contribute to any form of esthetic experience. Accordingly, in addition to vision and hearing, there are discussions of tactual, olfactory, gustatory, kinesthetic, and organic (including sex) esthetic response.

It is clear that no single volume could possibly contain more than an outline of the scientific aspects of these categories, and the reader who expects a Helmholtzian treatment is doomed to swift disappointment. On the other hand, the book is provoc-

atively rich in suggestions and problems for further experimental study. Among these may be mentioned the relation of the feeling-tone of colors to specific materials, and to specific and relative brightness values and the "illusions" of transparency, illumination, perspective, etc., as related to physiological and perceptual behavior. For example, there are three types of space involved in painting: geometric, produced through knowledge of Euclidian geometry; atmospheric, which must reproduce the colors and density and "distance" of the air; and finally, illuminative, which must distinguish "glow," lustrousness, and lighting, as very distince aspects of perception and therefore of painting technique. These in turn are related to the point-by-point relative reflection and absorption of the various parts of picture. As examples of these principles, the author presents illustrations from the various schools of painting.

In the auditory field, an effort is made to establish analagous categories. Thus there are really three comparable acoustic "perspectives" which follow the initial localization of a sound in space. Similarly, analagous acoustic 'glow,' lustre, and lighting are produced "quite obviously" by different musical instruments. Not all of the author's examples in this respect would pass muster by ensemble players, since the fusion of such timbres is quite another thing, perceptually, than are the individual tonal qualities. On safer experimental ground are his very good discussions of interval "quality;" tonality—fortunately including non-European systems; melodic principles; perception of acoustic rhythms, and harmonic 'laws.' In general, the discussion of music is the most satisfying part of the book, since it is rather more restricted to experimental data, and provides excellent illustrations.

In the later chapters, such general esthetic concepts as proportion, contrast, number, symmetry, repetition, economy, and "esthetic geometry" of various lines, patterns, and parts, are further discussed and illustrated from the various fields of art. Finally, the unifying principles which produce the basic "Gesamteindruck" (Gestalten of a higher order) receive both scientific and some speculative treatment.

In terms of critical estimate, the book has the following defects: a forced imbalance between the enormous subject matter and its technical analysis; a tendency toward provincial selection of examples—not always first-class—in the visual arts; and a tendency toward the same introspective dogmatism of which he accuses the philosophers, especially where scientific data are scarce or lacking. On the other hand, the book must be regarded as an important synthesis of the material of the practical arts, the problems of analytical esthetics, and the contributions of physiological, classical, and configurational psychology. It is especially valuable in bringing together the various experimental studies of the Gestalt school and relating them directly to perception of esthetic form. The book is entertainingly written, and at least to those with sufficient scientific and artistic background to fill in the gaps, it is provocatively stimulating.

University of Michigan MARTHA GUERNSEY COLBY

Die Formenwelt des Tastsinnes. By G. RÉVÉSZ. Haag, Martinus Nijhoff, 1938, Vol. I, pp. xii, 291; Vol. II, pp. x, 293.

The theory of haptics embraces the principles of perception and knowledge as well as those determining the creation of forms in the world of tactual impressions —the world of forms as it appears in the spatial consciousness of the blind. Many isolated studies of the psychology of the blind exist, but up to the present time

these remained unsystematized and no clear distinction was made between the results of speculation and the findings of empirical research. The consequence was a certain indefiniteness and the presence of many contradictions in the theory of haptics or the science of tactual phenomena, in marked contrast with the consistency and definiteness achieved in the fields of optics and acoustics.

Dr. Geza Révész, formerly professor of psychology at the University of Budapest, and now at the University of Amsterdam, is well known in America for his *Psychology of a Musical Prodigy,* and other studies in the psychology of music. He has devoted many years to psycho-physical researches in vision, audition, and tactual sensation. This has led to a preoccupation with the general problems of space perception and its various modes in man and animals. The present volume on the world of forms experienced by the blind, is a natural outcome of studies of this character and although the author is modest in his claims it will doubtless prove a revelation to most readers. Révész employs a variety of approaches, philosophical, mathematical, biological, and experimental, and discusses the significance of haptic experiences for varied aspects of life, but particularly in art.

Volume I treats of the basic principles of tactual perception and begins with a general discussion of the sciences of space. The author is a nativist and assumes the a priori nature of the intuition of space. An analysis of the relation of optics to haptics and of the latter to geometry introduces us to the theoretical principles of haptics, *i.e.* the tactual perception of form. The chief empirical problem in this field is that of the concrete nature of tactual space and its objects. It appears that there is a complete contrast in the structure of tactual and visual perception. The former is characterized by an analytical emphasis of parts in a whole, while the latter obeys a configurational principle achieving a unity of spatially articulated parts.

Haptic and optic space are the basic forms of space perception and space as perceived by the other senses is always a derivative. Visual, tactual and kinesthetic space, though equally complete in their development are of unequal importance, because they do not yield equal amounts of knowledge concerning the external world.

Haptics may be general or special and in the section on the special haptics of form, the author shows that the perception of tactual forms depends upon ten general principles and tendencies, viz. the steoreoplastic principle, the principle of successive procedure, the kinematic principle, the metric principle, intentional attitude, the tendency toward schematization, the tendency toward transformation, the principle of structural analysis, the principle of constructive synthesis and the principle of autonomous configuration. All of these principles and tendencies are deduced and demonstrated from experiments with normal and blind subjects, the latter again including subjects congenitally blind as well as subjects acquiring blindness at various ages. Experiments and observations on cases of operation for the removal of blindness are included. The second chapter on special haptics of form discusses the knowledge and total experience of proportion and symmetry while the third chapter, deals with varieties of tactual form perception, as determined by the predominance of either the optical or the haptic experience. Révész characterizes such types of tactual form perception as optomorphic or haptomorphic according to the data, dominating their construction. Volume I closes with a brief section on the perceptual world of the blind, in which several approaches to a practical psychology of the blind are suggested and the basic problems for investigation are formulated.

In the second volume of his work Révész develops the esthetic problem of the

tactual world. He asserts that this is a virgin field and he offers the first scientific report on the creative achievements of blind sculptors and modelers. This volume which is in some sense an independent work, is richly illustrated and must prove of great interest to all who are concerned with the welfare and education of the blind.

The first part is devoted to the general principles of a tactual esthetic, the difficulties attending esthetic perception on the part of the blind, and the esthetic evaluation of plastic art by means of the tactual and kinesthetic senses. The esthetics of the blind are found to depart from accepted standards, and their plastic creations tend to symbolize emotional urges rather than represent reality as normal individuals perceive it. The second section dealing with the creative activities of the blind, presents comparative experimental studies of the work of the congenitally blind, persons who have developed blindness at various ages, and normal individuals, working with bandaged eyes. There is an interesting account of the creative activities of blind sculptors of the past such as Kleinhans, Vidal, and Masuelli. The volume closes with a chapter on the basic problem of the blind in achieving artistic form, and the significance of sensory limitations in determining types and styles of plastic art. The author declares that a complete and adequate discussion of haptics is beyond the powers of a single individual but expresses the hope that he has laid the foundations and formulated the basal concepts of a genuine psychology of the blind and an explanation of the nature and significance of haptics in the elaboration of our perceptual world.

This hope seems justified, since the volumes certainly represent the first complete organization of the subject and, though occasionally burdened with theory, suggests many new approaches and problems and presents a rich fund of specialized information and excellent bibliographies.

University of Cincinnati CHARLES M. DISERENS

Taste and Temperament. By JOAN EVANS. New York, 1939. Pp. 128, 48 illustrations.

This is a curious little book in which the author applies Jung's well known types *extravert* and *introvert* to the perception of art by layman and artist. Not satisfied with this simple bifurcation, the author proceeds to "subdivide these categories according to the pace of their reaction, and so arrive[s] at a classical quaternity of temperaments" which she calls "slow" and "quick." Useful as Jung's types have proved themselves to be, they are a classification of opposites with a wide ranging middle ground. When to these two types are added the qualifications "slow" and "quick," one is at a loss to know just what is meant. Though the author is at pains to elaborate her characterizations, they remain indefinite because they so obviously overlap.

For instance, the slow extravert is deliberate, persevering, dogmatic, methodical, academic. He is a collector "who in seeking for Rarity falls in love with Rarity for Rareness-sake." He is mathematical, geometrical, favors straight lines and Aristotelian classifications. He is apt to confuse beauty with use. Though courageous, he is haunted by fear and tends toward megalomania and paranoia. He is dynamic, shuns decoration; a moulding or a cornice repels him. His interest in the association of ideas might make it hard for him to believe "that none of this associational process enters into a purely aesthetic sensation." Yet "the existence of 'surrealist art' which is composed of an expression of such associated ideas" complicates the ques-

tion. "Fortunately," concludes the author, "an art which declares itself without aesthetic intention lies outside the scope of this book." Baroque is an outstanding type of slow extravert art, and the Escorial is a monumental example. Typical artists of this persuasion are Michelangelo, Titian, Holbein, Rembrandt, Delacroix, and Goya.

The author finds somewhat less to say in characterizing the other three types. The quick extravert is feminine, understands the feelings of others, and is exemplified in the actor, salesman, courtier, and the great mystics. All extraverts think of their surroundings as backgrounds, the slow type as autobiography and the quick as *mise en scène*. Color interests them more than form. Dreams and illusions, the supernatural and sentimentality engage them. They are dissatisfied with the world, view and paint it in peculiar perspectives, and their figures levitate. Botticelli, Raphael, Tintoretto, Correggio, El Greco, and Wiliam Blake are cited as quick extraverts. Among the quick introverts Wiliam Morris, John Ruskin, and Sir Joshua Reynolds are cited as exemplars. These theoretical persons are yet said to be genuinely capable of an esthetic emotion, both disinterested and profound. This type favors decoration, landscape, and still life. The compositions are static, simplified, and make much use of receding planes of distance. In contrast to the slow extravert, the slow introvert favors curved rather than straight lines, and in contrast to the quick extravert his figures show weight rather than levitation. The slow introvert is harder to find and to recognize. He is more discrete, more driven to understand what he is doing. His philosophy is Platonic rather than Aristotelian. He is defeatist, given to psychological portraiture and cubic, box-like composition. Hugo van der Goes, Vermeer, Velasquez, Dürer and Cezanne are named to illustrate the type.

A sense of lightness, conveyed by whatever means, among which color would surely be one, appears to characterize all the examples selected as "quick," and a sense of heaviness all those called "slow." Thus, whereas the author's reference of these types of composition to Jung's classification of temperament and "the pace of their reaction" is odd, the esthetic sensibility that accounts for her divisions seems sure. A psychological problem is thus set which deserves further study.

Cornell University R. M. OGDEN

Psychology of Individual Differences. By A. R. GILLILAND and E. L. CLARK. New York, Prentice-Hall, 1939. Pp. xvi, 535.

There are colleges and training schools for educators, social workers, and business administrators, where the curriculum is built to specification according to the "needs" of the students. These needs have been formulated by some eminent educators, who write as if their information came from the Holy Ghost. These institutions demand certain courses, (1) which can be called psychology; (2) which look practical, being unfettered by 'theory' or by logic; (3) which prerequire nothing but semi-literacy of the students; (4) which demand little of the students except recitation of certain words in grammatical order; (5) which are 'certified' by the authorities who license teachers to teach; and (6) which also will be passed by at least 95% of the students who elect them. Such courses demand specially written text-books.

Our authors, however, have sought "to collect in one volume the more important facts and conclusions in the broad field of the psychology of individual differences" (p. v). In their chapter entitled "The problem of individual differences," they actually state no problems for solution. They do assert, repeatedly, and con-

vincingly, that every individual differs in at least some respects, from at least some others. Moreover, these differences are recognized by intelligent individuals, who differ from each other as widely as Aristotle and waterfowl. They assert (pp. 1 f.) that wild ducks, as well as wild geese, fly in V-formation; that the foremost bird is also the leader of the flock in the sense that he has been "selected" probably for his physical prowess, and also in the sense that he sets the course for the rest of the flock. Some ducks, of course, do not fly in this manner. As for the geese, who can tell whether the foremost bird sets the course for others, they for him, or external conditions for them all? I have seen flocks of wild geese change their direction, and at ~~the same~~ time change formation and also "leaders." Of course, as Aesop taught us, we should try to imitate the wisdom of animals, but I am not sure that the geese have set us a perfect example.

As "causes" of individual differences, the authors mention age and sex, but they emphasize heredity and environment. Which of these latter is the more important? They cite authors who have expressed opinions, as if the question were not meaningless, like the question, which side of a rectangle has the more to do in determining its area. They mention C. B. Davenport and L. M. Terman as believing that at least some mental traits of mankind are inherited according to Mendel's law. Why did they not examine this belief? Mendel's law is limited in its scope. It presupposes two incompatible traits, unequivocally defined, and practically discernible. It presupposes the mating of two individuals from "pure stocks." It presupposes the mating of brothers with sisters in the first filial generation. It is, therefore, quite irrelevant to censuses of Kallikaks, Jukes, and Edwardses.

Why, also, did they not question the tradition about Jonathan Edwards? He was married 212 years ago, and had 12 legitimate children. By conservative rules of guessing, how many descendants may we suppose he has had by this time? How many of them, probably are old enough, or have been old enough, to have attained distinction? How many of them have been identified? Does the number who have been identified make up an abnormally large part of the probable number of adults? And are not those who are distinguished (for good or bad) more likely to be identified than the mediocre?

Again, consider a member of the 6th filial generation: he has 64 ancestors of the degree of Jonathan Edwards. Suppose he himself is eminent, and that one of his ancestors in the 6th degree is Jonathan, while the other 63 are not. Why regard Jonathan Edwards as a sufficient cause of his greatness, and ignore the other ancestors? It seems to me that somebody ought to analyze and weigh the premises in the eugenists' propaganda, instead of re-patterning them to uncritical undergraduate students.

In their chapter on the measurement of differences, the authors leave undefined certain statistical concepts, such as standard deviation and correlation, which underlie much that follows. They pay due ritualistic honor to the normal distribution, and give roughly the same misinformation about it as mental testers usually give. "Long ago," they tell us (p. 68), "it was observed that height and many other linear measurements of the body, if collected from large groups of men, would *tend to fall under* a normal curve" (italics mine). On pp. 112 f. they present 26 distributions of heights and 26 distributions of weights among 10,560 children classified by Bird T. Baldwin according to their ages. On p. 114 they say that "for the most part, the curves conform fairly closely to a normal distribution." From mere in-

spection, I doubt that this holds true. For amusement, Mr. Martin Kaplon and I have tested the normality of some of the other distributions which they present, by computing the normal distributions which have the same parameters as the actual, and by the X^2-criterion, finding the probability P that the misfit in each instance would occur by chance. One can indeed describe the distributions of the test-scores of 903 students (p. 64) and also the weights of Montague and Hollingworth's 1000 boys and 1000 girls at birth, within a tolerable error, by mentioning the parameters of the corresponding normal distributions. Incidentally, the tabular exhibit of these weights does not agree with the corresponding graphs on the same page (111); neither do the tables yield exactly the average weights that the authors mention. But our authors show some other distributions which one could not plausibly call normal. For the heights of Montague and Hollingworth's new-born boys (p. 110), the original data yield $P = 10^{-5}$; and for the girls, $P = 10^{-5}$. They might have mentioned that Boring (*Science*, 52, 1920, 464 ff.) reported in 1920 that the heights of 221,819 insured men misfit a normal distribution, P being about 10^{-60}. On p. 115, they show graphically Davenport and Love's distributions of the heights of "the first million" (actually 868,445) American soldiers drafted in the World War, and also of the same soldiers' weights. From the original data, we find for heights $P = (10)^{-1425}$; for weights, $P = (10)^{-39730}$, in which the exponent is uncertain by at least 5. One recalls, of course, the exhibit which Yule reproduced in many editions of his monumental work, of the heights of 8585 men from the British Isles, fitted beautifully to a normal curve. This distribution, however, is *not* homogeneous, being made up of four components, namely English, Scotch, Irish and Welsh, which, according to the X^2-criterion, cannot be plausibly regarded as having been drawn from the same parent population. Moreover, their numerical strengths are *not* proportional to the populations of the political units from which they are drawn. Thus, it is *not* generally true that the heights of large samples of men are normally distributed, or nearly so.

Earlier (p. 69) the authors suggest that a mental test should be rejected if its scores are not distributed normally, although it should not be accepted merely because they are. Some mathematicians find such assertions appalling. On p. 391, we are told that the Moore-Gilliland Aggressiveness Test has a "reliability" of 0.75, but a "validity" in estimating ratings of 0.90. According to some educational statisticians, this assertion implies that the test-scores "measure" something else better than they measure themselves.

This test and certain others are, according to the authors, indirect and imperfect measures, but nevertheless useful measures, of existing traits. But there *cannot be* such a trait as General Aggressiveness, such that its measure can be denoted by a single number for a particular individual, indicating "how much" aggressiveness he most probably will manifest in any unspecified situation. "To aggress" is a transitive verb; it is meaningless without an unequivocally defined object. A similar assertion holds true for such fictitious traits as General Intelligence, General Persistence, General Gullibility, and the like.

On pp. 88 f. the authors marvel that the so-called mental ages of children younger than 15 yr. are simply proportional to their chronological ages. The secret is, of course, that the testers "planned it that way." They selected their test questions so that the average mental age of the children in their "standard" population would be exactly equal to their chronological ages, as nearly as they could accomplish that

feat; hence, the average of the intelligence-quotients of the standard children of any age has to be exactly 100. Had the authors exhibited the defining operations of mental age and intelligence-quotient, neither they nor their readers need to have been mystified.

Without searching for misprints and errors in citation, one finds a good many in this book. On p. 252, the authors speak of "tetrachloric" correlation. On p. 6, they misquote the Declaration of Independence, call E. H. Weber a psychologist, cite Fechner's *Elemente der 'Psycho-Physik,'* and specify 50% instead of 84% correct responses as the criterion of threshold-discrimination according to Fechner's method of right and wrong answers. They also cite Thurstone, L. L., and *Thelma Gwinn* (p. 18), but Thurstone, L. L., and Thurstone, T. G. on p. 371; they mention Gall and *Spurtzheim* twice on p. 120; Henry *F.* Garrett (p. 95); A. A. *Robach* (pp. 356, 396); *Lela* S. Hollingworth (p. 313); Mohr and *Grundlach* (pp. 344, 348); *J. J. B.* Watson (pp. 26, 57). They ascribe a certain monograph to May and Hartshorne on p. 314, but to Hartshorne and May on p. 396. Perhaps one should not cavil at details, but if they are numerous, one wonders if they may not be symptomatic.

My chief interest lies in the principles according to which the factual material is organized. From what I have said it should be clear that they are at least debatable.

For such courses as I described at the beginning of this note, this book may gain many "adoptions" immediately. If it has other uses, I do not now identify them.

Tulane University H. M. JOHNSON

Logical Aspects of Educational Measurement. By B. OTHANEL SMITH. New York, Columbia University Press, 1938. Pp. vii, 182.

This book should be required reading for every student of psychophysics, psychometrics, and educational measurements. It is disturbing, and it is therefore important. The reviewer disagrees with practically every important conclusion which the author draws, but on trying to find specific flaws in the arguments advanced, and in trying to establish his own views on a rigorous logical basis, he finds extraordinary difficulty. Dr. Smith has raised important fundamental questions. His answers are unsatisfactory. So are the answers attempted by the present reviewer! The questions, therefore, remain, and they are important and fundamental.

The author's argument, briefly, is as follows. Measurement implies the application—and the applicability—of certain elementary mathematical operations to data. The data must be shown experimentally to lie on a qualitative continuum and to exhibit properties corresponding to the postulates of the mathematics before measurement is proved possible. The postulates in question are those of *order, equality,* and *addition.* Fundamental measurement is possible if all three sets of postulates are met. Derived measurement is possible if the postulates of order and equality, but not those of addition, are met. Educational tests attempt to measure learning outcomes. These do not lie on a qualitative continuum. Learning integrations involve more than the sum of the elementary experiences out of which they are built. Two students who achieve the same score by answering different items on a test are not thereby shown to possess equal learnings, even after allowing for errors of measurement in the usual statistical sense. The nature of the interactions among experiences and learning outcomes renders extremely dubious the process of assigning to an individual a total score consisting merely of the *sum* of his item-scores. The normal

distribution has never been adequately demonstrated to apply to mental traits, and normal curve scaling has not been shown to produce equal-unit scales. Progress in measurement is to be looked for in two directions: (1) through factor analysis methods, which *may* succeed in isolating simple, abstract, linear aspects of the complex of learning outcomes; (2) through a more careful study of "real learning outcomes," leading to the possible development of complex measuring instruments, and scoring schemes which actually parallel in some sense the organization of these "real learning outcomes."

It is impossible in the space of a short review to criticize the book adequately, and, as noted above, the reviewer is not yet satisfied with his own criticisms. Only a few general comments will, therefore, be made.

In the first place, it is doubtful if test builders have as strong a belief that they are measuring "real learning outcomes" as the author implies. They assume, more nearly, that they are measuring *one aspect* of these outcomes, which is correlated positively but imperfectly with the other important aspects. Test experts are among the first to insist that the validities of their instruments are lower than the corresponding reliabilities, but that does not mean that these validities are so low as to render the instruments valueless. On the contrary, the universally positive correlation between test-scores and competent subjective estimates of "real learning outcomes" provides some demonstration of the validities of tests.

In his discussion of learning, the author makes much of the fact that "real learnings," which affect the self, are to be distinguished from superficial learnings and conditioned reflexes, which do not. He mentions but does not stress the fact that these latter are forgotten much more rapidly, and implies that many tests tap primarily these superficial and rote learnings. This would be true only if each test were given immediately following instruction, and if the items called primarily for reproduction of the formal elements. By using items which require discrimination and reasoned understanding of the facts, and by collecting these into comprehensive examinations which are given some time after the formal instruction is over, a great part of the criticism is met. This is precisely what the test experts have been advocating—and to some extent practicing—for a decade or more.

In his treatment of the foundations of educational measurement in general, and of the normal distribution in particular, the author exhibits unfamiliarity with certain fundamental statistical principles not found in the elementary texts. He seems, in particular, not to know the Taylor-series argument for the first-approximation validity of linear combinations of test items, nor the fact that the normal distribution has been derived without the assumption of equal, all-or-none, additively-combining elements. A great many of his criticisms lose some of their force in the light of these mathematical demonstrations.

In spite of shortcomings in the analysis and solution of the problems raised, however, this book should be read, because it is the first, so far as the reviewer knows, which states these problems adequately, or even fully recognizes their existence.

American Council on Education EDWARD E. CURETON

A History of Science, Technology, and Philosophy in the Eighteenth Century. By A. WOLF. New York, Macmillan Co., 1939. Pp. 814; 345 figs. and plates.

The present volume—which after four years follows its excellent predecessor for the sixteenth and seventeenth centuries (reviewed in this JOURNAL, 48, 1936, 695 f.)

—maintains the standard, style and arrangement of the first volume. Science is the chief subject-matter; technology is important but secondary; philosophy is included as a courtesy to a relative of science. Thus we have in this book: Science—mathematics, mechanics, astronomy, physics, meteorology, chemistry, geology, geography, botany, zoology, medicine; Technology—agriculture, textiles, building, transport, power plants, machinery, steam-engines, mining and metallurgy, industrial chemistry, lenses and specula, mechanical calculators, telegraphy; Philosophy—psychology, social science, economics, philosophy proper.

This volume, like the first, holds to a golden mean between that brevity which lists names, dates and events and those long expositions which are thorough at the expense of something else. The treatment is not so brief but that it provides insight into the development of discovery and thought. Thus for the most part it becomes unreasonable for the reader to complain about omissions, since any addition would necessitate either the deletion of some other item or the writing of a longer book—and surely it is a reasonable undertaking for Professor Wolf to try to write a book on eighteenth century science and thought in not more than 772 pages.

As in the first book, so here there is a short chapter toward the end called *Psychology*. It is twenty-seven pages long and deals with Berkeley, Hume and Hartley for several pages each, then briefly with Diderot, Condillac, Bonnet and Cabanis, then still more briefly with Tetens, Wolff, Kant, and Moses Mendelssohn. It spends a page on Pereire's teaching of deaf-mutes. The chapter is clear and for the most part too elementary for psychologists.

Is there any psychology to be found under *Physiology?* No, there are only four pages on physiology: Haller, Réaumur, Spallanzani, and Hales. The beginnings of reflex action with Robert Whytt (1751) are not mentioned, nor his anticipation by Astruc (1736). That was a trivial affair in eighteenth century science, but what is Professor Wolf going to do about a background for reflex action when he comes to the nineteenth century?

Under the physics of light we get accounts of the conflict of the corpuscular theory with the wave theory, of Euler's contribution to that problem, of Bouguer and Lambert on photometry, of Robert Smith's *Compleat System of Opticks* (1738). The psychologist might have been interested in being told further that Bouguer in his photometry anticipated Weber's Law, that Lambert invented a color pyramid as well as a photometer, that the first certain case of color blindness was described by Huddart in 1777 and that Dalton's famous color blindness, which gave rise later to the term *daltonism,* was described in 1798. There is also the large literature on after-images and contrast for which Plateau cited in 1878 more than a hundred papers dated before 1800. The more important names here are Buffon, Benjamin Franklin and Robert W. Darwin. These are omissions that the psychologist would have liked to have supplied, but I can not tell Professor Wolf what else he should have omitted in order to let these in.

The physics of sound is very briefly treated: Sauveur on beats and consequently on the calculation of frequencies; Euler on the frequency formula for strings; Sorge, Romieu and Tartini on combination tones; Chladni on his vibration figures and the velocity of sound; Priestley on the effects of different media on sound. Sauveur's limits of audibility are given as 12½ and 6400, Euler's as 20 and 4000. I do not think that there are important omissions, although the invention of the tuning fork by the bandmaster, Shore, is interesting (1711).

As a matter of fact there really was very little scientific 'psychology' during the eighteenth century, in spite of Professor Wolf's saying: "Eighteenth century psychology showed a marked tendency toward independence as an empirical science. A number of works were written on various psychological subjects without any admixture of philosophical or theological discussions" (p. 668). He is speaking here of Berkeley's *New Theory of Vision* and of Hume's *Treatise,* and certainly these authors were more empirical than many of their philosophical colleagues. Nevertheless their contributions lack the faith in experiment and observation that characterized Newton's discovery of the complexity of white light, or, for that matter, Benjamin Franklin's account of how negative after-images ("spectra") give place to positive when the eyes are closed. All of which is not to say that Professor Wolf has not used his great skill and wisdom in the completion of another masterpiece of erudition.

Harvard University E. G. BORING

Gemeinschaft und Herrschaft als Staats- und Kulturtypen. Ein kulturanthropologischer Versuch. By PETER VON WERDER. Stuttgart, Ferdinand Enke, 1938. Pp. viii, 150.

It can be argued that the rich anthropological materials have not been sufficiently utilized for sociological theory, and as far as the author purports to use them for sociological typology one would welcome his attempt, though he tends to identify too readily the social conditions among the contemporary "primitives" with the "early" form of human societies (pp. 3 ff.). It is more than doubtful, however, whether the two concepts of socio-political organization thus derived—"gerontocratic community" and "patriarchal domination"—can be considered as sufficient for the analysis and interpretation even of those "early" associational formations. The inadequacy of the conceptual equipment of the author becomes still more apparent when more complete and infinitely variable historical and contemporary forms of dominational associations are approached (pp. 41 ff.).

The author, however, credits his two fundamental types of political domination with the ability to serve the interpretation not only of the legal forms, administrative systems, and military organization of any social group, but even of the whole range of social activities, art, science, and so forth (pp. 98 ff.).

Quite arbitrarily the two basic types of political organization (gerontocratic community and patriarchal domination) are thus reduced to two "types of family" and, in the last resort, both are related to racial types as expressive of their "natural" tendencies (pp. 11 ff.), though the author himself seems to waver somewhat in taking this unsubstantiated position.

In every concrete society the author sees either gerontocratic community or patriarchal domination or a mixture of the two. Their "harmonious mixture" is the "ideal" political form, only rarely actualized, and it goes without saying that the author finds it, among a few other cases such as the Athens of Perikles and the *Preussischer Ordensstaat,* in contemporary Germany.

The two types are not clear, precise, and unambiguous in themselves, though this is the first requirement of an 'ideal-type.' The curious thing, however, is that the negatively evaluated form of "patriarchal domination" seems to be approximated by the reality of the German situation much more closely than "gerontocracy" or an "ideal mixture" of both.

No doubt, the author is a gifted young man. He has intuition, constructive imagination, and happy *apercus* which could serve as useful hypotheses. He makes some good points, for instance in his sociology of art and knowledge (pp. 88 ff.), but good ideas remain without elaboration or substantiation and are presented as dogmatic assertions.

The book is a classic example of the disastrous blinding effects that a political ideology produces on scientific theory and research. Nothing could be recommended to the author more urgently, in his own interest as a young scholar, than to free himself from these ideological bonds, to look around him for sober empirical facts, and for more useful theoretical typologies in the field of political sociology. In the work of Franz Oppenheimer, Max Weber, Georg Simmel, Tönnies, Pareto, Mosca, MacIver, and others, there is available nowadays a considerable wealth of historically well substantiated and serviceable constructions.

Columbia University A. VON SCHELTING

Psychological Factors in Marital Happiness. By LEWIS M. TERMAN. New York, McGraw-Hill Book Co., 1938. Pp. xiv, 474.

The problems of marital incompatibility, domestic felicity, and conjugal adjustment are obviously psychological but they are not problems which have been much studied nor are they problems which are easy to attack. Because psychologists are so often called on for advice (even teaching) in these matters any study, especially one by a person so competent, is peculiarly welcome. The present volume reports the results of an effort to break into this difficult field. It is, however, essentially a pioneer research monograph. The marriage counsellor and the person involved in marital difficulties will not find much in the book to assist him.

Obviously the first thing to do was to measure degrees of marital happiness. For this a device was developed which is a combination of the rating scale and the questionary. With this in hand it was possible to correlate degrees of marital happiness with scores on personality measures and to compare in some of the various possible ways groups of happily married with the unhappily married. Both of these procedures were used, but in the main study on 792 married pairs, measures of personality traits were not obtained. The technique was to select from the Bernreuter and the Strong tests only certain questions and to add to these many others which the investigators thought desirable. However effective this method, it unfortunately ruled out the possibility of discovering the significances of the various possible Bernreuter and Strong scores. Perhaps this was done because a preliminary study did not reveal much significance in such results. The author and his associates argue that the sex relation is actually but a small part of married life; nevertheless their list of questions includes many items concerning sex practices. Much of the book involves the presentation of how the groups differ in terms of answers to specific questions, although one may properly question the reliability of information obtained by a single question on any subject.

The results are on the whole not especially startling. The happily married group were found to have had happier childhoods and happier parents, to have been subject to less severe discipline, to have had less conflict in adolescence with their own parents, in temperament they were more conformative and in reference to family size to have been a little less often 'only' children. It was just a little surprising to learn that comparisons of the happily married with the unhappily married did not show differences in the amount of education as a matter of much

significance. Nor were differences in age a matter of much importance. In general it might be said in summary of many items that the happily married were of the more conservative variety of people.

Data from so large a number of adults would be valuable under almost any circumstances, but data so carefully obtained as these are genuinely worthy of very serious attention. A very clever plan was devised whereby husband and wife wrote their answers without possibility of influencing each other and then by another device which continued to maintain anonymity the returns of husband and wife were kept together without anyone knowing the identity of the respondents.

Doubtless the author and his associates would be the first to agree that the problems of marital happiness have not been solved by this study. Certainly no one could do much counselling in terms of what has been found here for the obvious reasons that these results are presented in averages. It may be quite true that a difference in amount of education may not be on the average a significant matter in the prediction of marriage success but it is quite possible that in an individual instance the degrees of education might be very influential. The same may of course be said of many other items included. The very important "will-to-coöperate" was not measured and is probably of prime importance. Perhaps the recollections of childhood reported are not valid. Such recollections are notoriously unreliable and here they may have been colored by the present degree of marital happiness or unhappiness. Any reader may feel that the measure of marital happiness used does not measure what the reader thinks of as marital happiness but it is a carefully prepared device and readers have only their own feelings to go by.

It is further possible that some of the background items stressed are important because of the traitis of personality produced by them and that these traits of personality should have been studied. It seems to the reviewer quite unfortunate that the methods prevented the determination of Bernreuter and Strong scores in the major study. But such things are easy to think of afterward. That the subjects for the study were groups of persons living in California and therefore perhaps not typical, the author readily admits. Obviously what is needed now is a follow-up study which shall sample populations far and wide over the country and by still better methods than those used for this. And, if possible, still other traits should be measured with testing begun long before the initiation of the marriage venture.

Indiana University EDMUND S. CONKLIN

The Happy Family. By JOHN LEVY and RUTH MUNROE. New York, Alfred A. Knopf, 1938. Pp. 319.

This is an application of common-sense psychiatry to making happy families. It is not an over-simplified or diluted psychiatry, but one which sees the intricacies of family relationships, yet has large trust in wholesome living and common sense as over against the application of elaborate psychiatric theories.

Dr. Levy, who seems chiefly responsible for the writing, was an associate professor of psychiatry in the College of Physicians and Surgeons, Columbia University, and died just before the book was published; Dr. Munroe, his wife, was formerly professor of psychology in Sarah Lawrence College. The book is delightfully written in a conversational style, with a fine dash of humor, and woven around composite case studies. Some of the ethics might seem dubious to the more conservative, but they are entirely wholesome.

There is probably nothing very new in the book for the psychologist or psychia-

trist familiar with the literature of the family, but there is a profound wisdom in the book which will be challenging to the best professionals in this field.

The authors stress the "unconscious motivation" involved in the relations between husband and wife and parents and children, and the need for their recognition. The character of the person rather than the immediate circumstances are of most importance in controlling his behavior. Marriage problems have two facets: the real difficulty and the psychological meaning of that difficulty for the person involved. Without minimizing the importance of sex adjustment, they feel that it has been overemphasized, and that it is not so important a cause of marital maladjustment as many have claimed. They consider the need for love and self-fulfilment as the chief drives in marital relationships, but point out that the desire for dominance and protection alternate in every personality and are not uniform in time.

The common-sense point of view of the book is shown in the following summary statement with regard to infidelity: "Married people will do better, I think, to follow the course of procedure that seems to them individually natural and right rather than to base their decision on elaborate theorizing" (p. 100).

This book is a good antidote for over-sophisticated or over-conscientious parents who have been unable to digest their reading of current literature; is good reading for every student of the family; and is recommended for psychiatrists who take themselves too seriously.

Cornell University DWIGHT SANDERSON

The Sex Criminal. By BERTRAM POLLENS. New York, Macaulay Co., 1938. Pp. 211.

Bertram Pollens is the senior psychologist of the Penitentiary of the City of New York at Rikers Island, where a "Sex Clinic" has been established to study intensively the 700 cases which are classified as sex offenders out of the total of 18,000 commitments each year. According to Richard A. McGee, Warden at the Penitentiary, "This book is the first well organized, authoritative, and readable effort to bring the subject up to date for the general reader."

Pollens emphasizes throughout the book two points: (1) That the traditional legalistic attitudes, which disregard the psychological antecedents, should be discarded; and (2) that the sex criminal is characterized generally by his emotional immaturity. On the first issue, society is taken to task for its punitive rather than therapeutic attitude toward the sex criminal. On the second, Freud's convenient psychic compartments (Id, Ego, and Super-Ego) are uncritically accepted and introduced as explanatory concepts.

Some of the topics discussed in the book are: "Infantile Sexuality," "Homosexuality," "Is the Sex Deviate Born or Made?," "Is the Sex Criminal Insane?" The concluding chapters deal with the disposition of the sex criminal and advice to parents. Some interesting conclusions are put forth in the chapter entitled "Sex Criminals as a Group," for example, that most sex criminals are recidivists, that there is no real increase in sex crimes, that in a group of sex criminals studied in New York 83% of them were over 30 yr. of age, and that although sex criminals are not as a group classifiable as feebleminded they are usually of below-average intelligence. Pollens suggests that the only proper method of dealing with the sex criminal at present is to segregate him from the community, to place him in an institution which is neither penal nor correctional, so that he may be super-

vised, studied, guided, and, if possible, treated. The institution should be in the nature of a mental hospital rather than a jail and psychiatrists and psychologists should have the responsibility of supervision and treatment. The book is replete with case studies, many of them of recent tabloid notoriety. Only the male offender is considered.

The book will probably prove of interest to some laymen and may also have value to parole officers, social workers, and institution authorities. Its point of departure is sociological rather than psychological. The psychological substance of the book is on an elementary level, as might be expected from the fact that it is intended for popular consumption. The emphasis is expository rather than critical. It is distinctly not a contribution to science. It is filled with localisms and slang, sometimes in poor taste. The author is unguarded in proposing generalizations as truth in the absence of experimental substantiation, as witness the following statement: "Scientists, who specialize in microscopic work have substantiated their peeping instinct, while physicians who derive pleasure from the practice of surgery have degenitalized their sadistic trends."

Although the book contains information of value to some readers, its acceptance by psychologists is limited by the lack of verbal precision. Careless proof-reading and lack of adequate organization are added handicaps to its usefulness. A greater degree of scientific objectivity and impartiality, even in a popular treatise, might be expected from a psychologist. Further revisions prior to publication were in order.

Rochester, New York ANTHONY J. MITRANO

The Behavior of Organisms: An Experimental Analysis. By B. F. SKINNER. New York, D. Appleton-Century Co., 1938. Pp. ix, 458.

In this book Dr. Skinner attempts the construction of a positivistic, descriptive system of behavior. There are no hypotheses, the various terms used in the exposition being non-explanatory, and merely descriptive of carefully defined modes of behavior. New terms are devised, not through any desire for the esoteric, but simply because common terms imply a *conceptual* system, and are thus more than purely descriptive. It is interesting to note here the departure from those writers who use old terms, but with new definitions.

In the study of behavior, 'running accounts' are not desired, since they are difficult of quantification. Rather, the basic behavioral unit is the "reflex," which is defined as a given environmental circumstance and a reliable response. Thus, there is a "lawful connection" between stimulus and response, but there is no attempt at the prediction and control of behavior, since this implies "botanizing," or the collection of reflexes. Rather, the attempt is to work out the general characteristics of all behavior. A further interesting point is the author's dichotomy between "respondant" behavior, which is directly elicited by an environmental situation, and "operant" behavior, which seems spontaneously emitted by the organism. He points out that the attempt to force behavior into a stimulus-response explanatory system has delayed its adequate scientific treatment.

After a list and discussion of "dynamic" and "static" laws of the reflex, the bulk of the book is given over to the presentation of the results of several years' experimental work by the author upon behavior in the rat. A standard situation, in which the animal by pushing upon a lever could obtain a pellet of food, was used throughout. This is not the place to summarize Skinner's results, so suffice it to say that an extreme degree of careful control was used, and the results present

a remarkable degree of consistency. There is no attempt to be exhaustive—that is, not *all* drives, for example, were considered. Instead, the author attempted to arrive at generalizations adequate for drives in general, and for organisms in general.

In the penultimate chapter, it is pointed out that neurology is not appealed to. This is done for four reasons. (1) Neurology is not actually more orderly than behavior. (2) Neurological data do not 'explain away' behavior. (3) More actual data concerning the nervous system are needed. (4) A precise behavioral description is needed before neurological correlates can be demonstrated. Furthermore, Skinner points out that 'CNS' should be read 'conceptual nervous system' in such a system as Pavlov's, since hypothetical centers and connections are used as explanatory media.

In the final chapter, the systematic approach is summarized. The system is mechanistic and analytical, each of which terms the author stoutly defends. The emphasis is upon quantitative data. Further, operant behavior is pointed out as being most important, since this makes up the bulk of adult behavior. Finally, there is no attempted extension to human behavior, although Skinner sees no differences, except in complexity and verbalization.

The reader of Skinner's book is left with the impression that, whether he agrees or not, the author has done his job well. The exposition is very clear and, in the reviewer's opinion, unambiguous. By anyone who feels that the future of comparative psychology, especially, lies in the greater objectification of its descriptive categories, this work should receive a ready welcome. The emphasis upon operant rather than respondant behavior should be hailed by all who object to the forcing of behavior into falsely simplified explanatory systems. Perhaps the specific laws will be modified with the extension of research, but surely the basic position should always deserve respectful consideration.

University of Alabama F. NOWELL JONES

Mental Therapy: Studies in Fifty Cases. By LOUIS S. LONDON. Vols. I and II. New York, Covici-Friede, 1937. Pp. viii, 774.

The author makes excellent use of the case method to show the relation between the neuroses and psychoses and to illustrate the technique of psychoanalysis in psychiatry. The neuroses, or psychoneuroses, including the paraphilias (perversions), are classed as minor psychoses "to differentiate them from the psychoses proper, often referred to as major psychoses." The 50 cases are drawn from the author's own practice and include a wide range in the field of psychopathology. The narrative form of these case reports makes possible for the reader a sense of progress in each analysis. He sees not only the patient and his symptoms, but also the analyst at work.

The author's interest is primarily that of the psychiatrist. He follows Stekel, with whom he worked in Vienna in 1932, in asking, "What can psychoanalysis and my experience do for the patient?," rather than asking with Freud, "What does the case bring for science?" In general, the author agrees with Stekel in both theory and practice. In Chapter II he gives a brief survey of trends in psychoanalysis. He concludes that "the two schools, the orthodox Freudian school and the Stekelian, which had been diverging for the past two decades, are now approximating."

Part V reports cases of schizophrenia and paranoia; Part VI, cases of manic depressive, or cyclic, psychoses. In Case 39, the author reports a favorable outcome

in an advanced case of schizophrenia after four years of psychoanalysis. "Not only," he concludes, "has he (the patient) made a satisfactory adjustment to heterosexuality, but he has resumed his studies and will eventually study commercial chemistry. Nevertheless, there is still a question as to the practicability of analyzing cases of dementia praecox." He is equally cautious about exaggerating claims for psychoanalysis in the case of manic depressives, but urges further exploratory analysis in both of these major psychoses as likely to throw light on psychogenesis and etiology.

Psychologists will find these case studies valuable source material in abnormal psychology, and particularly in the psychopathology of sex. Part III includes 17 cases of paraphiliac neurosis, or sex perversion, and there are examples of paraphiliac trends or sexual maladjustments in a majority of the other cases reported.

The implications of these case studies for a program of mental hygiene which includes prevention as well as therapy are not discussed by the author, but should be pondered by any reader who is interested in education, in the applied fields of psychology, in court or welfare work, or in public health, as well as by the psychiatrist. One is impressed by the geometric ratios involved in social contacts. Each of these cases is reported as an individual. Yet each of these individuals had significant contact with many others. If Case 10 indulged as a child in sex play with three, four, or five other children, these three, four, or five presumably each had similar experiences with others. In the same way there is a widening circle in the case of the perversions. It is seldom that they are passed on by one individual merely to a single partner.

There is also in these considerations an implication for the psychiatrist himself in the matter of social responsibility. There is the analyst-patient relationship, as in the treatment of disease by the physician, but there is also a public mental hygiene responsibility. The physician recognizes a public health responsibility when he reports a communicable disease and quarantines his patient. How can the psychiatrist show himself equally responsible?

Dr. London closes his second volume with a glossary. It is easy to criticize a definition, but often hard to write one. Psychologists will be likely to criticize the following unfavorably: "*Conscience:* That part of our mental faculty which deals with the moral sense of right and wrong." "*Hypnotism:* A form of therapeutics where a person is forced into an unconscious state by command of the therapist."

Ohio State University FRANCIS N. MAXFIELD

Hearing: Its Psychology and Physiology. By S. S. STEVENS and H. DAVIS. New York, Wiley, 1938. Pp. xv, 489.

The authors attempt to present an inventory of the recent discoveries in the psychophysiology of hearing, and to determine to what extent auditory research is able to yield a consistent point of view. Of the 330 titles listed in the bibliography, 280 have dates of publication more recent than 1927. No attempt is made to orient the respective topics historically. Other deliberate omissions are the fields of applied acoustics, speech, phonetics, noise and music. The book is divided logically into two sections: the first nine chapters deal with the psychology and the last nine sections with the neurophysiology of hearing.

The first part of the book begins with an analysis of the nature of the auditory

stimulus and is followed by a treatment of pitch, loudness, volume, density, localization, harmonics, fatigue, vibrato and beats. The second part is concerned with an elementary presentation of the anatomy of the ear, following which is a short chapter on bone conduction and deafness. Preceded by a section on neurophysiological principles, the next two important chapters deal with aural microphonics ("cochlear effect") and the rôle of the basilar membrane. In the authors' opinion, the best explanation at the present time is some form of a "place" theory. The concluding three chapters deal with the nerve impulses in the peripheral and central acoustical pathways. Much significant material concerning the relationships between the cochlear effect and true auditory potentials is presented and constitutes one of the more important sections of the book.

To some extent, criticism of the choice of references, amount of discussion allocated to the various topics, etc., depends upon the predilections of the particular reviewer. In general, the studies surveyed are good, although one could argue for the inclusion of a number of recent important studies which have been omitted. From a student's point of view, reference is made to the work of some 12 investigators for which no bibliographical sources are given. There are also about 21 citations in the list of References (of which 17 were reported since 1927) which are never mentioned. Since a source-book was not the objective, these references should have either been omitted or else discussed in the text. In several instances, experimental studies are improperly cited.

What of the book as a whole, the avowed purpose of which was an inventory of recent discoveries? If the volume is accepted as an expression of the work and viewpoint of the Harvard Laboratories, it does accomplish its objective. This does not mean that only studies from this laboratory are discussed, but that the Harvard data have been emphasized. As such, a contribution has been made which no serious student of the field can overlook.

Often the discussion consists simply of an abstract of a particular experiment, the value of which would have been materially enhanced if the treatment had been more critical. For example, the analysis of auditory localization seems to be quite weak. While comparisons are perhaps invidious, the same topic treated by Woodworth, (*Experimental Psychology,* 1938, 518-534) is a more scholarly and organized presentation. While sufficient time has elapsed since the first studies of cochlear microphonics in 1930, the results of many investigations do not seem to have clarified several vexing problems. Speculation is offered in the interpretation of specific phenomena, but this did not go far enough. Since the purpose was to "determine to what extent auditory research is able to yield a consistent point of view" one can only conclude that either the authors missed their goal badly, or else a coördinated account is not possible at the present time. They state that "theories flourish on a certain sparseness of facts and wither in the face of abundance" (p. x). One can take issue with this remark or not, but one good hypothesis is worth a dozen uncoördinated facts.

The decade initiated by the report of Wever and Bray in 1930 has brought about a remarkable development of our knowledge of the electrical phenomena of the peripheral acoustical mechanisms. Our understanding of the central acoustical system has not progressed at the same pace. Anatomically there is the work of Nó, Poliak and Walker; studies of the electrical responses of the brain stem by Kemp et al.;

functional studies of the medial geniculate bodies by Ades, and that of the component tracts by Culler and his associates. Beside the studies of Kornmuller in 1937 and Pfeifer in 1921 on cortical electrical functions, this about covers the pertinent experimental data on the central acoustical system. It is possible that further advance in our knowledge awaits the acquisition of more data on the cortical auditory pathways and their rôle in hearing.

From a pedagogical point of view, this book is neither an introductory text nor a handbook. In the former capacity, several sections are far in advance of anything now available. The treatment of the properties of the auditory stimulus, the electrical techniques and instrumentation should be of considerable value to the student. A clearcut distinction between the properties of the stimulus and the psychological attributes is very ably presented. The relationship between intensity and frequency in determining both pitch and loudness is likewise handled very well. The student will also find a good statement of the neurophysiological principles and their significance for hearing. Many sections are far beyond the introductory student. It also follows, that some sections are too simple for the scholar. That the book is not a handbook is not intended as a criticism, since this objective was clearly not the authors' goal. However, as stated in the introductory *Perspective* by one of the dedicatées, "Certainly we are now ready for a new *Lehre von den Tonemfindungen* to orient us" (p. vii).

Brooklyn College EDWARD GIRDEN

Elements of Psychology: An Introduction. By ALBERT CLAYTON REID. New York, Prentice-Hall, 1938. Pp. xix, 409.

To write a textbook of psychology from a frankly elementaristic point of view requires nowadays considerable courage. For this reason, if for no other, Professor Reid is to be complimented. He has adhered loyally and openly to the tradition of the late Professor Titchener. In the arrangement of chapters, even in the arrangement of topics within chapters, he has deviated only slightly from the plan of Titchener's *Textbook of Psychology;* and his style is characterized by a directness and lucidity which shows the influence of the Titchenerian model. The student should find the book clear, interesting and not too difficult.

The reaction of the professional psychologist to the book will probably be one of disappointment. It is like an echo from the past. One gains the impression, not that the psychology of the past twenty years is insignificant, but that the past twenty years have never really existed. Much of the book might, in fact, have been written in the days of Helmholtz or Ebbinghaus. One cannot quarrel with a writer for adhering to a conservative point of view. One even has a certain lurking respect for an experimentalist who finds little dignity in chapters on motivation, personality and the testing of intelligence. One must insist, however, that even an introductory textbook pay at least some attention to the current research in the field which it purports to represent. A psychology of elements still constitutes, in theory at least, a defensible point of view. It has been vigorously challenged both on theoretical and on experimental grounds. It is a pity that Professor Reid, as one of the few remaining Titchenerians, has not attempted to meet that challenge.

Swarthmore College R. B. MACLEOD

You Can Sleep Well. By EDMUND JACOBSON. New York, McGraw-Hill Book Co., 1938. Pp. xix, 269.

Those who are acquainted with Jacobson's earlier publications on progressive relaxation will not be surprised to find that the present volume contains a popularized extension of his program to the induction of sleep. The author has gained in literary fluency with the passage of time; his device of making "The Doctor" the protagonist who conducts conversations with the reader definitely aids in maintaining interest in his exposition. On the systematic side, one senses a definite change on only one point; namely, that Jacobson is now apparently convinced that relaxation can be learned in somewhat less intensive fashion than he had previously maintained.

The lay reader will probably most enjoy the earlier chapters with their fluent description of the varied means by which Everyman courts sleep. Psychologists will read with greater interest the two chapters which review attempts that have been made to measure sleep and the theories which have sprung from these attempts. Those who are not concerned with the psychology of sleep may find reason for reading the volume in Jacobson's treatment of the relationship between muscle tension and various types of imaginal activity. Certainly all psychologists will recognize that Jacobson has not failed to use his literary device to take certain covert digs at some of his contemporaries. Even the dullest reader will not fail to mark the author's persistent effort to disclaim any connection between the effects of progressive relaxation and those of suggestion.

University of Maryland JOHN G. JENKINS

New Trends in Group-Work. Edited by J. LIEBERMAN. New York, Association Press, 1938. Pp. xii, 229.

Of more interest to educators and social workers than psychologists, this book is a collection of papers from varied sources. The two avowed objectives of the book are to encourage "self-critical thinking and experimentation" in the field of group-work. The latter objective apparently is considered important by the editorial board for they say, when seeking appropriate contributions, "we were astonished at the almost complete absence of material describing experimental group-work efforts, and we found very little material on group-work processes in activity and group-relationships." The implication would seem to be that all of the studies on the social behavior of children lie outside of the field of group-work. The social psychologist, however, may well feel that this is a rationalization for the "inspirational" approach to problems in the field rather than one of controlled observation.

If the contents of this book are a sample of the efforts of group-workers, "self-critical thinking" among them is certainly a necessity. This may be exemplified by a low point in the book where one author, after what is apparently intended to be a description of Pavlov's experiments on the comatose state induced in dogs during trace conditioning, asks the rhetorical question "Are we not justified, then, in concluding that all frustration (*sic*) tends to create corresponding inertia and that normal functioning is thus interrupted?" The outstanding article is one by G. B. Watson entitled "Virtues versus Virtue," a truly critical analysis of procedures in character education. In the reviewer's opinion this article should be read by every one working in the field of character education.

University of California EDWIN E. GHISELLI

Some Factors Affecting Resumption of Interrupted Activities: By Preschool Children. By EVELYN KATZ. Minneapolis, University of Minnesota Press, 1938. Pp. ix, 52.

This monograph reports the results of the author's experiments on the resumption of interrupted activities by children. The experiments are notable for the extreme care exercised by Dr. Katz in controlling the conditions surrounding the original tasks and the presentation of the test situations. The study is mainly concerned with the effect on resumption of the stage of the original task at the time of interruption, and on the presence or absence of alternative tasks. The monograph is well written, and the comments are cogent. The student interested in this topic will find the introductory remarks concerning past work, and the conclusions especially interesting.

University of Alabama F. NOWELL JONES

La function de l'imagination. By RENÉ LACROZE. Paris, Boivin et Cie., 1938. Pp. 159.

This is the sequel and companion-piece of the author's longer treatise, *L'angoisse et l'émotion,* in which was advanced the not un-Spencerian thesis that discomfort, disquietude, or uneasiness is the primordial root of all emotion. Disclaiming any intent to revive faculty psychology, and assuming the functional approach to be complementary to the structural, Lacroze defines function as the orientation of the organism toward a definite end, through some more or less stable psychological synthesis. The problem as to whether imagination is purely a gratuitous operation, subserves explanatory or practical needs (as in myth or magic), or has merely an autistic function, is then attacked.

In a long argument, running through eight chapters, and illumined by citations ranging from Comte, Lang, and Lévy-Bruhl through Janet, Ribot, and Bergson to Claparéde, Dumas, Sartre, Dalbiez, and the less extravagant disciples of Freud, Lacroze supports (rather than arrives at) the conclusion that the function of imagination in dreams, in religion, in metaphysics and in art is purely subjective and personal, the fortifying of the inner self against the sense of human impotence. Art and religion, in this view, afford temporary respite from anticipated and inevitable failure, by way of mass emotion, or brief illusion of the vulnerability of material forces to human need, through prayer or magic.

The exaggerations of imaginative function and the disorders to which this may lead are touched upon, though a contribution to normal rather than abnormal psychology is obviously the aim. In the chapter on Symbolism there is reference both to Freudian theories, and to the French symbolist poets; to the relation, also, of imagery to thought.

Much of the argument is on the a priori or arm-chair level, revealing in the opinion of the reviewer little intimate knowledge of primitive man or the child mind. The social function of imagination, its interpretative aspects, its rôle in invention, its daily service in the enrichment of life, are minimized or overlooked. The task of imagination is defined in terms of the thesis of the earlier volume, the fundamental disquietude of existence, for which it serves as anodyne. According to Lacroze, the primary service of imagination is the fictitious realization of desire, offering salutary outlet to the many impulsions, spontaneous expression of which

is incongruous with a complex civilization. The complete story of the imaginative functions is obviously yet to write.

These twin volumes serve, however, a secondary purpose. Not only does their prevailing pessimistic tone illume the 'introvert' temperament of the author and certain of his countrymen, notably Pascal, Renan, Maurice de Guerin; it reflects also the disturbing implications for the life and aspirations of the individual of the present European scene.

Though this second monograph is a contribution to theoretical rather than empirical psychology, the breadth and sweep of the discussion, in fluent and melodious prose, offers refreshing contrast to much of the merely expedient and mediocre product of the psychological press. The typography of the volume is excellent. A bibliography of 142 titles and a helpful index of citations is appended.

Cornell University ELSIE MURRAY

Psychology of Child Behavior. By ARNE S. JENSEN. New York, Prentice-Hall, 1938. Pp. xxi, 664.

A recent survey of the teaching of psychology in state teachers colleges indicates an interest in a greater emphasis on the growth and development of children, adaptive and adjustive behavior, and techniques for the control and modification of behavior. Jensen's book on child psychology is an out growth of class experience and reflects this interest and trend in the table of contents. After an historical introduction in Part I he devotes Part II to an examination of the child as an organism with chapters concerned with physical, motor, and mental growth, heredity, native equipment, learned responses, and individual differences. In Part III he treats of the child as a social being in the expressive arts, play, intellectual life, the emotional life, behavior problems, and group life. Part IV reflects principles of child guidance, including a discussion of mental hygiene, personality development, and the hygiene of behavior. A final division (V) gives an orientation of the child in relationship to scientific fields and to the future. The method of organization involves some duplication of theme in Parts III and IV.

Planned primarily as a textbook, the treatment is elementary in style and content as contrasted to recent books in the field. For example, Chapter IV on Physical, Motor, and Mental Growth is presented without a statistical table, graph, or reference to a recent scientific journal. Suggested reading and references are to other books rather than to current periodical literature. The elementary nature of the book is reflected in its list of general references which emphasizes material from popular publications, such as *American Mercury, Ladies' Home Journal, Atlantic Monthly, Science News Letter, Good Housekeeping,* and *Time.* Jensen does not utilize directly current material from such organizations as the Society for Research in Child Development or place much reliance on technical psychological, medical, or biological journals. Journal writings are sometimes quoted by secondary reference to other textbooks.

The book will probably find some use in institutions offering courses in child psychology without prerequisites for students who will not be expected to develop an interest in current research.

University of Michigan WILLARD C. OLSON

Prolegomena in Psychologiam. By GIACOMO VAN DER VELDT. Rome, Libreria del Collegio di S. Antonio, 1938. Pp. 308.

Father van der Veldt, Professor in the *Ateneo Pontificale di Propaganda Fide* at Rome, belongs to the Minorites, an Order noted for an interest in the problems of psychology which extends back to the Middle Ages—to Robert of Lincoln, Roger Bacon, St. Bonaventure, Duns Scotus. His book offers, in Latin, a Preface to psychology; since Latin is still an international language in schools of the Roman Catholic clergy, the book may well enjoy considerable popularity. The choice of Latin, however, has brought the author difficulties; on occasion, as he says, he has had to sacrifice Latinity for clearness, and to borrow words from the modern tongues. Thus we are prepared to meet neologisms like 'behaviorismus,' 'functionalismus,' and 'configurationismus' (*Gestalt*).

The volume, intended as a manual or a book of reference for students, is divided into four chapters. The first discusses in some detail the aim and scope of psychology, and in an Appendix treats, from both the doctrinal and historical points of view, the relation of the natural sciences to psychology. The second presents an historical survey of psychological thought from the Greeks to the present time, but not neglecting primitive and Oriental, psychologies. The treatment here is perforce brief. Plato receives only seven lines, Patristic and Mediaeval Psychology five pages, Experimental Psychology in America six pages. The inclusion of a section on history in a textbook on psychology the present reviewer counts as a virtue.

Chapter III describes the manifold divisions of contemporary psychology, distinguishing the systems according to aim, scope, and method, and in addition considers the special fields of psychology. In order to illustrate the author's method of subjoining brief appraisals to his descriptions and expositions I translate two short sections on psychoanalysis: "*Merits:* It has contributed much to our knowledge of the processes lying hid in the psychic depths. Not only does it inquire formally into desires, longings, and instincts, but it also reveals their real, individual aspect. Rightly and deservedly does it extol psychic dynamism, and lay bare the flow of instincts into the phenomena of consciousness. For psychiatric studies it proves exceedingly fertile. *Contra:* Though it may truly make numerous observations of a most subtle kind, yet many of its claims are gratuitous, and even more of these are greatly exaggerated. Its sexual theory concerning the importance of the instincts in the psychic life is surely exaggerated, and in a number of points has by no means been proved. The applications it makes in the fields of religion, theology, pedagogy, and art, resting as it does on the doctrine of sublimation, are sometimes impious and perilous, and often ridiculous." The final chapter studies methods, general and special, including measurements.

The work has an orderly arrangement. The interpretations are clear, and the criticisms, despite the dangers involved in simplification, are generally fair. A selective bibliography is affixed to individual sections. In the General Bibliography I miss Bentley's *Field of Psychology;* and in the list of periodicals the *American Journal of Psychology* is listed under its former editorship.

Cornell University HARRY CAPLAN

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